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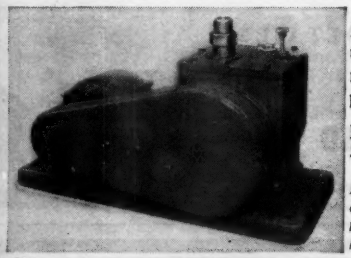

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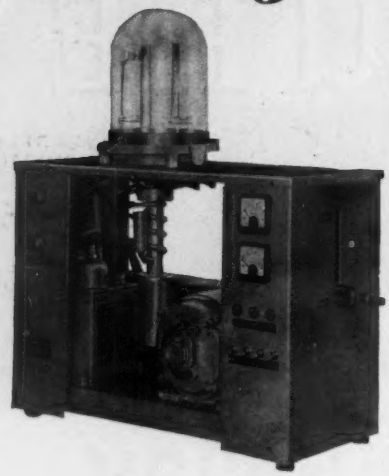

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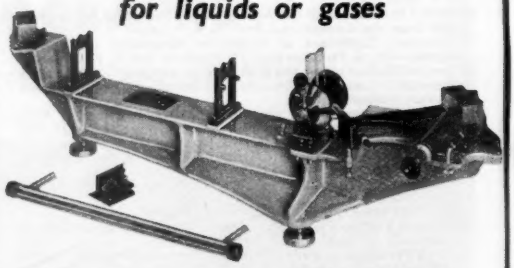

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ARTIFICIAL EARTH SATELLITE, 1958 ALPHA

ON October 4, 1957, the launching of the first artificial Earth satellite, 1957 Alpha, was announced (*Nature*, October 12, 1957); and this was closely followed by the news that the second artificial Earth satellite, 1957 Beta, had been launched on November 3, 1957 (*Nature*, November 7, 1957). Both these *Sputniks*, as they were then called, were successfully launched, presumably from Russian territory.

Now comes a third announcement to the effect that the third artificial Earth satellite, Alpha 1958, at present known as *Explorer*, was successfully launched from Cape Canaveral, Florida, on January 31.

1958 Alpha is apparently travelling on a larger orbit than that of either 1957 Alpha or 1957 Beta, for it is taking twenty minutes longer to encircle the Earth than the 1957 satellites. The reported minimum height is about 200 miles and the maximum about 1,700 miles. Its mass is about 18 lb., and that of the final stage of the rocket, after burning out, is estimated to be 12·67 lb. Both have been designed to travel along the orbit as one unit.

Because 1958 Alpha, having a total mass of about 29·7 lb., is small compared with 1957 Alpha and Beta (184 and 1,118 lb., respectively) the instruments which it carries are also miniature, but they will probably yield results as valuable as the larger ones. The new satellite is a 6-in. tube about 80 in. long. Apart from radio transmitters, it carries instruments for measuring temperatures, cosmic rays and the effect of space dust on its outer surface.

Once again, therefore, it is *Nature's* pleasant task to convey to the scientists concerned the congratulations of all their colleagues throughout the world. Science and technology are advancing in great strides, though clearly the launching of these three satellites, like the control of thermonuclear fusion, are just the beginnings of developments in space technology on one hand and atomic technology on the other with all their scientific and technological complications. Yet now is the time to draw morals and learn lessons from these achievements which, without doubt in the none-too-distant future, will exert profound impacts on man and his civilization.

During the past few months, three great achievements in science and technology have justifiably received world acclaim: (1) the launching of artificial Earth satellites by scientists in the U.S.S.R.; (2) the development and control of thermonuclear fusion by scientists in Great Britain and the United States; (3) the launching of an artificial Earth satellite by scientists in the United States. We have used the expression 'by scientists in' deliberately, for the implications and interpretations already attached to these great scientific achievements, especially by the more sensational or politically biased newspaper Press, may do much harm to scientific reputation. It is therefore important to recognize that not one of these great achievements can be looked upon as wholly Russian or American or British. There is every reason for believing that the great work which led to the launching of the satellites in the U.S.S.R. was carried out by teams which comprised some scientists of Western birth and/or training. Among those who have brought about the control of thermonuclear fusion in the United States is one of British birth and training, whereas among those in Great Britain are an Australian and a Canadian. Germans are outstanding among the scientists who were responsible for the successful launching of the satellite from American soil.

Some branches of science, especially the physical sciences, are becoming more and more the tools of diplomatic intrigue and political pressures. Certainly, scientific achievement is being thrust forward quite sensationally in the mad rush for national prestige. Yet scientific discoveries are not suddenly made by men who, by birth or consent, are entitled to claim a certain nationality; they are the results of good training, patient and sometimes long-continued work often fraught with disappointment, team-work sometimes spiced with real personal genius, and all these are based on the work of previous generations. The nationality of the scientists plays no part whatever, though we must give credit to those countries which, in their wisdom, give financial and other support to such scientists. It is therefore of the utmost importance at this stage to recognize that men born and trained in the United States, Germany, the U.S.S.R., Canada, Australia, New Zealand, Great Britain and possibly other countries have teamed up and produced these recent gratifying results; and it is about time that credit should be given to these scientists and due recognition awarded to them, instead of to the countries in which they happen to be working.

This false 'nationalism' of science can do nothing but harm in the long run, for it tends to force scientists under still stronger political control (thus gradually depriving them of that freedom under which science has always flourished best) and has already led to ill-informed interpretation and presentation, and not a little acrimony. All this is due mainly to fear of giving away secrets to a potential enemy (in spite of the fact that science moves so fast that what is a 'classified' secret to-day becomes 'unclassified' world news to-morrow); anxiety to enhance national prestige (on false premises); and the determination of politicians and Service brass-hats to keep science under their control in order to achieve their own

short-sighted objectives. In this false presentation of scientific achievement, this artificial moulding of public opinion, truth (the main objective of science itself) becomes the first casualty.

It may be of interest now to reveal that, after the 1957 satellites were launched, British and American scientists willingly came forward with their observations and results for publication to the world. Some of these appeared in *Nature*; yet, in spite of long-continued efforts on the part of the Editors, nothing could be obtained from the Russians themselves, though the work was supposed to be part of the Russian contribution to the *International* Geophysical Year. In fact, the only contacts that could be made were with Soviet diplomats (and these contacts proved sterile). Two direct communications to one of the leading men of science in Moscow who was closely connected with the scientific work involved still remain unanswered. This compares unfavourably with President Eisenhower's pronouncement that results from 1958 Alpha will be given to the world, and reveals a deplorable reluctance on the part of the Russian authorities to allow their scientists to work in collaboration with scientists in other countries. This international race in science may achieve some good in that it might enhance respect for science among political and other authorities and the public; but by and large, it is not good for science, which has always flourished best in an atmosphere of freedom and collaboration. Moreover, it is bound to cause duplication of scientific effort (and nowadays scientific research can be a very expensive item in any nation's budget); this is a waste of time which must inevitably slow up world advancement of science. The fact that this is not recognized by some is indicated in one British newspaper's leading article commenting on the launching of the third Earth satellite when it asks: "Why is Britain left helpless and earthbound in this conquest of space? What are our leaders doing?", and gives the answer as "utterly shameful".

Similar reactions occurred when the results of research on the control of thermonuclear fusion were announced. It is true that the scientists working in Britain were ahead of those working in the United States, and this is now recorded in *Nature* and elsewhere. We are in no position to express any opinion on the criticisms hurled at American authorities and the accusation that they deliberately planned the holding up of announcements from Britain for the sake of their national prestige. In any event, in the long run it will not matter who was first in the field, provided, for the sake of history, credit is given to those who have earned it. But the immediate effect of such international squabbling can be very harmful and distressing. Moreover, it inspires ill-feeling and bad faith where neither is deserved; and this all too often leads on to misunderstandings, false accusations and even the deliberate masking of the truth. For example, quite unknown to the Editors at the initial stages, it was agreed among the scientists concerned (of several nationalities) and others closely connected, that the first scientific details concerning the work on thermonuclear fusion in Britain and the United States should be published in *Nature*. This was indeed a compliment to *Nature*, though it was obviously realized by those concerned that, by publishing in this journal, the news and results would quickly reach most scientists throughout the whole world.

The articles were eventually published in *Nature* of January 25; but not before there had been some severe criticism from certain sections of the newspaper Press, mainly from writers who anyhow could scarcely be expected to understand and interpret what appears in *Nature*. A further criticism came along after publication, when *The New Scientist* in its issue of January 30 suggested that the Editors of *Nature* had substantially departed from accepted practice by omitting the dates from the articles published, and implied that thus they were helping those to claim priority which was not their due. Actually, throughout its long history, *Nature* never has published dates on its main articles. All seven of the articles on thermonuclear fusion were received at *Nature* office in the normal way within a few days of each other and treated in the normal way.

All this attempted 'nationalization' of science for the achievement of political prestige, this squabbling among Press and public concerning who came first, are very childish indeed. It must be disconcerting, if not discouraging, to the scientists; and, as has been seen, is bound to lead to distortion of the truth. The stark facts are that quite a number of countries are training scientists, some of whom have proved their worth and even genius in the past few months. The fact that their proving grounds were in the United States, the U.S.S.R. and Great Britain is, fundamentally, of little consequence to science itself, for, as we pointed out in our article congratulating the scientists in Russia on their magnificent achievement last October (*Nature*, October 12), there is no such thing as Communist science or American science or British science: there is just—science, the search for the truth. It is unfortunate that though many countries are now supporting this material search for the truth, too many of them are interfering with its exposition.

The *New York Times* of February 1, in commenting on the launching of 1958 Alpha, gives a firm warning that: "This is a good time for us to remember that neither Russians nor Americans can claim any kind of exclusive credit for the progress made towards man's conquest of space. Like all man's greatest scientific adventures, the march of humanity towards the cosmos is the product of many men of many nations over many centuries. . . . To realize the possibilities now before us we shall have to show that all humanity is capable of rising to the challenge". This can only be done by looking upon the march of science as an endless, moving picture in which many characters have a part to play; where star actors can give of their best only when they are supported by a good and well-trained cast; and where, as occasion demands, praise or blame be accorded to the actors for their acting and not their place of birth.

TECHNOLOGICAL HUMANISM

THE address given by the Duke of Edinburgh in Manchester on November 22 when he opened the new extensions to the Manchester College of Science and Technology included some welcome comments, and it may be reasonably hoped that his lucid and cogent argument will bring home certain truths and facts to quarters which have long ignored them. The dependence of the welfare and security of Britain upon an adequate supply of properly trained and educated scientists and technologists could scarcely have been better put. Nor did the Duke mince his words. Lagging production, he said, is properly equated with chronic poverty, and he gave neither encouragement nor opportunity to those who advocate excessive specialization or argue that it is inherent in a scientist or technologist.

Very clearly, but firmly, the Duke of Edinburgh pointed out that a clash between science and technology and the humanities is not inevitable, and his advocacy of technology does not lead towards technocracy. He insisted that a specialist of any kind, whether in science or in the arts, is not necessarily the well-educated man that we need to produce, and that it is doubtful whether sufficient science is included in the general education of the humanist for the latter to be described as well educated to-day. Science, or any other branch of knowledge, is only an educational tool the use of which depends on the character and outlook of the individual possessing it. Furthermore, he reiterated the truth that our limited scientific and technological man-power can only be efficiently and economically used by administrators who know enough to recognize for themselves the fields of research and development likely to prove most fruitful.

Apart from thus commending the attention being given in the College to management, the Duke of Edinburgh stressed three points which apply no less to the current expansion of technological and scientific education generally than to the particular developments at Manchester. First, there is needed the whole-hearted support and co-operation of industry as a whole, with money and equipment and in terms of part-time teachers. Secondly, there is required the understanding of individual industrialists in formulating their needs for people with specific qualifications, and then in seeing that, when trained, these graduates do, in fact, obtain posts which offer full scope for their qualifications and talents. Thirdly, scientists and technologists should seek to dispel the present unreasoning prejudice against their subjects.

Apart from this last point, on which the Duke of Edinburgh laid perhaps his main stress, his address emphasized the arguments of recent addresses by Sir Eric Ashby on technological humanism and by Dr. R. P. Linstead on an education for our times. The goal should be the production of a well-educated citizen, capable of independent thought and initiative. To this end, the full co-operation of scientists and technologists both as individuals and corporately through their professional associations will still be necessary to overcome prejudices which would hinder the development of a system of education capable of providing the nation with the administrators, scientists, technologists and technicians as well as ordinary citizens, who can take full advantage of the opportunities of to-day. Even in securing the full co-operation of industry with the universities and technological and technical colleges, there are prejudices to be overcome; indeed, it is difficult to believe they are not the real reason why the Manchester College of Science and Technology has had to curtail its plans for expansion, because of a disappointingly small grant from the University Grants Committee. The position may also be due to the anomalous arrangement under which the development of higher technological education in Britain is only partly the responsibility of the University Grants Committee; but in the present financial stringency, the removal of any anomalies which hinder the wisest and most economical use of Britain's limited resources should be a principal object of Government policy.

The Manchester College of Science and Technology is seeking to meet its difficulty by an approach to local industrial firms, which it is hoped will enable it during the coming year to wipe off its existing mortgage of nearly £1·5 million, and this appeal could well result in much closer co-operation and understanding between local industry and the College. Nevertheless, the contribution which the College could make to the expansion of technological education is so considerable and unquestioned that there are strong grounds for supporting an alternative proposal that the University Grants Committee should be authorized to make a capital grant towards the discharge of the outstanding mortgage.

The sincerity of the Government's intentions in regard to the expansion of technological education are, in fact, better shown by the support it gives to the three major institutions—the Imperial College of Science and Technology, the Royal Technical College, Glasgow, and the Manchester College of Science and Technology—than by the progress of the expansion on which Mr. Geoffrey Lloyd, Minister of Education, gave a further interim report in the House of Commons on November 28. Apart from the anomaly in regard to the University Grants Committee already noted, the plea of regionalism is scarcely consistent with the development of the Salford Technical College while expansion at Manchester is frustrated, particularly in view of the proposed devolution of responsibilities for training technicians and craftsmen which is proposed at Manchester. In all these circumstances, Mr. Lloyd's statement, so warmly welcomed in the House of Commons, is disturbing rather than reassuring. Besides eighteen new technical colleges and seventy-nine major extensions of existing colleges in Britain since February 1956, plans for a further

five colleges and seventeen major extensions have been approved and a further fourteen new colleges and twenty-eight major extensions are contemplated. Mr. Lloyd gave no indication as to the balance between technical and technological facilities, nor could he report on the technological developments at the universities, which, like those at the three major institutions already named, are outside his department. Indeed, as Sir David Anderson implied in his address in Dublin to Section G of the British Association, the country's requirements for technicians are scarcely known. It may be that, until the Carr Committee reports, the requirements for training craftsmen and technicians cannot be formulated with any precision, although a useful estimate could be made from the number of scientists and technologists which it is estimated that we require. It is on this basis, however, rather than on the programmes of other countries, that the expansion of technological and technical education in Britain should be considered.

It is most desirable that financial support for the expansion of the Manchester College of Science and Technology and other colleges of technology and technical colleges should be drawn from industry as well as from the Government and local authorities. That £100,000 should already have been received from local industry in response to the Manchester appeal (made in the first instance to meet the expected deficit in the current academic year) is a healthy sign. It is understood, however, that the College is likely to need £850,000 from industry during the next five years, and it is important to remember that the expansion of technological and technical education depends on co-operation from industry in other ways, some of which are even more important than direct financial assistance and may themselves be costly to industry.

This is apparent from the recently published report* of the second National Conference between Industry and the Technical Colleges arranged last May by the Federation of British Industries (see also *Nature*, **179**, 1226; 1957). Indeed, the report makes much more encouraging reading than Mr. Lloyd's statement, and it is noteworthy how far the ideas which Sir Eric Ashby, for example, had just expressed in his May Lecture to the Institute of Metals and which Dr. Linstead developed in his Hinchley Memorial Lecture in November were reflected on all sides at the Conference. There was overwhelming support for the view that the primary task is to produce well-educated men and women whether they are scientists or technologists or not, and Lord Hailsham's address at the formal opening of the Conference was far more reassuring than recent speeches of his successor as Minister of Education, Mr. Lloyd.

One point made by Lord Hailsham deserves to be widely noted. The Government would expect industrialists, he said, to play an important part on governing bodies of technical colleges, and this is the more important in view of the Government's proposals in regard to local government reform. The close co-operation between industry and technical education which is so essential can scarcely be secured unless industry itself comes to participate directly in the management of the colleges, and this came out quite as clearly in the subsequent discussions at the Conference on sandwich courses, as in Lord Hailsham's remarks. Moreover, in referring to the need for a two-way exchange of ideas between Government and industry to obtain a satisfactory solution to some problems of sandwich courses, Lord Hailsham stressed the bearing of an informed public opinion upon the financial question. Technical education, he believes, is at last capturing the imagination of the public, and if public opinion takes education seriously, there will be no difficulty about getting the money.

Another main discussion at the Conference centred around a challenging paper by Dr. P. F. R. Venables on "Liberal Technical Education: an Economic Necessity". Lord Hailsham himself insisted that a good general education is the first essential of a good technical education, and stressed the importance of a liberal element in technical education, which is the principal means of further education for most young people; he also pointed out that we have the difficult task of providing an industrial society which, by virtue of its moral structure, can lead the world to a smoother and more rational future. The divorce between the scientist and the student of the humanities must end. In an industrial age, everyone needs a general knowledge of the capabilities and characteristics of scientific investigation and mathematical discipline; but everybody, however specialized, needs also to recognize that this specialization is only part of a more general life in which the intellectual, moral, spiritual and æsthetic qualities have an essential place.

That such views should be expressed by a Minister of Education, as he then was, is in itself encouraging, and although Lord Hailsham said nothing about the importance of concentration of limited resources for the development of technological education, Sir Harry Pilkington, in his survey of technical education, at least recognized the need, and his figure of eight is a much more realistic number for the colleges of advanced technology that we can for the present expect to staff and equip adequately. Moreover, the question of quality becomes more important as we attempt to give effect to the ideas about a liberal technical education which Dr. Venables developed in his paper. Particularly during the period of transition, experiment and development into which we have entered, it is essential that adequate resources for experiment and for trial of new ideas should be assured in a few colleges rather than that available resources should be spread too widely. The most superficial development of Dr. Venables's reference to the contribution of the technical college library should indicate the immense possibilities inherent in a liberal policy to such libraries, and the way in which

* Federation of British Industries. Report of the Second National Conference between Industry and the Technical Colleges, 15 May, 1957. Pp. vi+71. (London: Federation of British Industries, 1957.) 5s.

it could react on adult education generally and facilitate a more effective use of resources.

Dr. Venables argued convincingly that a utilitarian philosophy is an insufficient basis for human beliefs and sympathies, for æsthetic experience, for human action and as a bond for society, and that it is therefore quite inadequate as a basis for education, knowing little of virtue and perhaps even less of creative imagination. It is probably the main factor responsible for starving the technical college libraries —and probably also those of universities and other libraries. But that is only one of the reasons why utilitarianism is a major danger against which we must guard in attempting to develop a broadly based education adequate for our times. The three main characteristics of a liberal technical education stressed by Dr. Venables—diversity of experience confronting the student, of subjects to be studied, disciplines to be mastered, and methods to be used; intensity of illumination of the student's mind and experience; and proper timing of the phases of education in relation to the student's personal development—all demand staff of high quality and open-mindedness, and conditions in which students have the opportunity to enrich their lives and develop their personalities.

All this bears, too, on the difficult questions of the selection of students and the amount and conditions of student grants, though not always in the way that the sponsors of the National Union of Students' recent survey of student expenditure might expect. Dr. Venables points out that the basic scientific subjects should be taken mainly in the earlier part of a professional course, though not to the exclusion of technological subjects, and that is a prime reason for liberal treatment of technological subjects. Moreover, if a diversity of appropriate teaching methods is to have its full effect, we must be concerned to see that the system of selection of students and awards does not in itself discourage the student from thinking and reflecting on his work, or hinder the strengthening of his powers of thought or the deepening of his penetration so essential either in mastery of a specialism or fostering a liberal education.

What is encouraging in Dr. Venables's paper and its reception by the Conference is the challenge to thought and the acceptance of that challenge by the Conference. It was recognized that full-time and sandwich courses provide the opportunity for experiment, and his assertion that we have relied too long and too much on part-time education was unchallenged. Sir Norman Kipping, in his summing up, showed as clearly as Sir Harry Pilkington the extent to which industry is prepared to co-operate and to work out the most effective means of co-operation. Moreover, he welcomed the Minister's stress on the promotion of education as distinct from its administration, and the address of Mr. A. A. Part, Under-Secretary of the Ministry of Education, showed that the permanent officials at the Ministry were just as alive to the real issues.

With such an enlightened attitude on the part of the Government and its officials, with increasing readiness on the part of industry to co-operate and to follow the lead of Sir Harry Pilkington, Sir Alexander Fleck, Lord Weeks, Lord Hives and others, and with educationists like Dr. Venables, Sir Eric Ashby and Dr. Linstead pointing the way, it may well be asked wherein the danger lies that the present plans may be ineffective or not realized. Despite the unsatisfactory character of the administrative arrangements, the University Grants Committee cannot fairly be blamed for deficiencies in the technological education of Britain. Lord Simon of Wythenshawe, in a valedictory address to the Court of Governors of the University of Manchester, recently remarked that the generosity of Government grants to universities is not always appreciated, and added that in a letter regarding the next quinquennium the Committee had emphasized the need to strengthen teaching rather than research and especially the teaching of technology.

It is true that, as regards the question of general education, the universities themselves could do something more to help the schools to make the necessary adjustments, and that there is room for better co-operation and clearer understanding at this level, though Dr. Linstead emphasized the delicate and sympathetic handling which these borderland problems require. The Duke of Edinburgh's speech, however, and the difficulties of the Manchester College of Science and Technology, point to the real need. First, there is the need for wider public understanding of what is required and what is at stake, and a deeper appreciation of the place and worth of technician, technologist and scientist alike. That is an essential condition for the creation of an informed public opinion that will ensure an adequate financial provision for education at all levels, from the Treasury or from local government sources, and also a continuing supply of students for technical and technological as well as scientific courses. This itself is in part a problem of adult education, but such an enlightened public opinion provides the ultimate means of overcoming the political, sectional and local prejudices which hinder the rational planning of resources and the elaboration of a national, regional and local system of education.

INFLUENCE OF WAR ON THE PRODUCTION AND USE OF FERTILIZERS

The World Fertilizer Economy

By Mirko Lamer. (Studies on Food, Agriculture, and World War II.) Pp. xvi+715. (Stanford, Calif.: Stanford University Press; London: Oxford University Press, 1957.) £5 net.

THE agricultural output of the United Kingdom increased greatly during the Second World War, mainly due to a large increase in the acreage under tillage crops. This change in farming resulted in a near-revolution in many of the techniques used; outstanding examples were marked increases in mechanization and in the use of fertilizers. The

War stimulated progress in many other countries, but in some others the use of fertilizers was seriously curtailed by war-time damage to plants or by restricted supplies. The main purpose of Dr. Lamer's book is to state and to discuss the changes in production and consumption of fertilizers which occurred during the war period in all the important fertilizer-using countries. Reviews of the fertilizer position in the pre-war and post-war period have already appeared for a few countries, but this book provides the first account on a world-wide basis of the industrial, agricultural, social and economic factors which were involved.

The first few chapters set the stage by giving general accounts of fertilizer history, the forms in which fertilizers are prepared, their role in plant nutrition, and their values for crops. Yield responses in relation to profit are discussed with special reference to the mathematical equations which have been derived for fertilizer response curves. There is little in this section of the book which has not been published previously.

In succeeding chapters fertilizer production and consumption are dealt with exhaustively, country by country; features of pre-war trade in fertilizers are discussed in relation to the changes imposed by the War and to the re-establishment of trade afterwards. An interesting section is devoted to the trade agreements which existed before the War between the producers of fertilizers in individual countries, and also in different countries. Most of the old cartels were broken up during the War, and the author discusses the possible establishment of trade agreements in the future. Changes in the forms in which fertilizers are produced, due to the development of new processes and to the exploitation of new raw materials, are described.

The patterns of fertilizer production and use in Allied and Axis countries are compared, and the effects on fertilizer use in neutral and non-belligerent countries are stated. Special attention is given to the arrangements made by the United States, the United Kingdom and Canada to promote co-operation in fertilizer production and trade for the benefit of all the Allies. Most countries controlled the sale and use of the fertilizer supplies that were available; the methods used for achieving this are stated and estimates are made of their success.

Two chapters are devoted to the use of fertilizers in the Soviet Union; they are particularly valuable since it is difficult to obtain any reliable information on fertilizer production and consumption in the U.S.S.R. Full documentation is given and the maximum use has been made of information from both official and unofficial sources. In 1913, Russia's fertilizer industry was practically non-existent—only a few hundred thousand tons were used and most was imported from Germany. Domestic fertilizer production rose from 180,000 tons (of products) in 1928 to 3·7 million tons in 1938. The War reduced output and the 1938 level was not again attained until 1948. Production in the U.S.S.R. in 1955 was estimated as 9·6 million tons of products of all types.

The book is concluded by a brief assessment of the future of fertilizer use in relation to economic, industrial, and agricultural factors. The author concludes that "in all countries of the world, fertilizers are one of the most important strategic weapons of modern agriculture"; he considers that: "Increasing food production by increasing the use of fertilizers seems often to be a better road to agricultural and national welfare in under-developed countries than building electric power plants, roads, and machine industries, and relying on gifts of food from well-to-do countries".

Full statistics for production, consumption and foreign trade are given and there is very adequate documentation throughout the book. The amounts of information available for different countries have, inevitably, varied and the account given is therefore fragmentary in places, and there are many gaps. A criticism is that related subjects are often widely separated in the text. It would have been easier for a reader to have had all information on the British Commonwealth or the U.S.S.R. in adjoining chapters; the layout adopted separates information on any one country or group of countries by a number of chapters dealing with other subjects. In spite of this the book is of very real value and interest, and is recommended to all who are concerned with the wider aspects of the manufacture, supply and use of fertilizers.

G. W. Cooke

SIR ERNEST SHACKLETON

Shackleton

By Margery and James Fisher. Pp. xvi+559+46 plates. (London: James Barrie Books, Ltd., 1957.) 30*s*. net.

DURING the years between the two World Wars, polar exploration in its heroic age held the palm for adventure; and many a schoolboy's hero was Sir Ernest Shackleton. I still remember the thrill when, not many years after his death, I found "The Heart of the Antarctic" in my preparatory school library and how I trudged every mile with him (by torchlight, under the pillow) when he got so near to, and yet turned back so far from, the Pole.

I next turned to "South", the account of his *Endurance* expedition, which, appropriately enough, must surely rank as one of the most extraordinary feats of human endurance of all time. More than forty years before Dr. Fuchs, Shackleton had conceived the idea of crossing the Antarctic continent from the Weddell Sea to the Ross Sea. Unfortunately, ice conditions that year were exceptionally severe and his ship became trapped in the pack at the base of the Weddell Sea just short of his landfall. How the expedition drifted north in increasing pressure until the ship was crushed like match-boarding before their eyes; how, after sledging across disintegrating pack-ice and then taking to the boats, the whole expedition landed safely on Elephant Island; how Sir Ernest himself, with five companions, sailed in an open ship's lifeboat across 800 miles of the world's stormiest ocean to South Georgia and crossed that completely unmapped island in search of help and how ultimately the entire party was saved without loss of life must surely remain an epic in leadership at its best.

Behind the explorer lay the man, and the demands of an expedition brought out all that was best in him—the fantastic physique, which enabled him to man-haul sledges for weeks on end in sub-zero temperatures on a twenty-ounce diet; the judgment which persuaded him to turn back when only a few marches from the South Pole; the improvizing genius which helped him to bring back his crew of the *Endurance*; the strong sense of the romantic without which no great deed is accomplished.

Back at home in settled life these gifts did not serve him so well, and whether it was mining concessions in Hungary, shares in a cigarette company or taxi business, or wild-cat schemes to search for hidden treasure, it was obvious that there was bound to be one outcome—yet another expedition to the Antarctic. To him, fulfilment could only come through polar exploration, and the tragedy too often occurring in such cases of an ageing man becoming more and more of a misfit in civilization, yet less and less capable of following his bent, was averted in Shackleton's case by his early death.

All these facts of a complex character and a detailed description of the four polar expeditions in which he took part, the last three as leader, are fully and, indeed, admirably set out in this definitive life, which is both timely and well written. Just as Wolsey and Roberts never disliked each other (indeed, they scarcely met, the rivalry was created by their respective staffs) so I think it is extraordinarily unlikely that Shackleton and Scott were so antagonistic as some of their more ardent supporters would have us believe. The evidence is examined scrupulously, and neither figure emerges with discredit.

The only fault of this book lies in its almost too-detailed approach. If ever there was a life of heroic achievement calculated to bring tears to the eyes, then surely it was Shackleton's. Yet it is not until his death in South Georgia, back again in his old haunts, chasing the rainbow with the same zest as twenty years before, that this tale ever becomes really moving, though through every one of the 559 pages there emerges one who was truly a man among men.

DAVID JAMES

SUPPLY AND QUALITY OF CRAFTSMEN

Recruitment of Skilled Trades

By Prof. Lady Gertrude Williams. (International Library of Sociology and Social Reconstruction.) Pp. vii+216. (London: Routledge and Kegan Paul, Ltd.; New York: Humanities Press, 1957.) 23*s*. net.

CONCERNED with the continuing shortage of skilled workers in industry, Lady Williams has made an inquiry to determine the reasons for the deficiency. Since most skilled workers are to be found in engineering, ship-building, the motor-repair industry, printing and building, the inquiry was confined to these industries and, since the number of skilled women workers is extremely small, it dealt only with male apprentices. Questionnaires were sent to some 9,000 firms and 550 of them replied; personal interviews were carried out in some of them. Making no claim for the statistical accuracy and significance of the inquiry, Lady Williams has still been able to substantiate her findings from many talks with individuals of wide experience in industry. All who are familiar with recruitment and training in industry and who love their country would like to deny the depressing story she tells; what she writes is all too true.

In the renowned engineering industry of Great Britain, for example, there are comparatively few firms with well-established training schemes for craftsmen run by competent people; the number of well-equipped training centres where teaching is the prime concern can be counted on the fingers of four hands. Where training is systematically carried out it is in the large firms which bear the responsibility of training not only enough skilled men for themselves but also for a very large number of small firms; many firms, including electrical firms employing thousands of workers, employ no apprentices at all. In the building industry the position is worse than in engineering, although Lady Williams concedes that the nature of the industry makes it extremely difficult to organize effective apprentice schemes. To promote interest in training young skilled building workers, the Government has encouraged the development of suitable training schemes which have had a little success.

In printing, less attention is paid to the training of apprentices than in engineering or building. It is rare for a firm to have anyone specially responsible for the training of apprentices, and, as Lady Williams observes, it may be that there are some firms in the printing industry with well-organized works schools but they did not come to light in the inquiry. "For the most part the apprentice is put in the care of an adult craftsman and it is a matter of chance whether or not his teacher is an interested and capable teacher."

The motor-vehicle repairing industry is preponderantly composed of small units, is comparatively new, and has no tradition of apprenticeship or other regulated entry to the trade. It has an attractive scheme of training drawn up not only to attract additional workers to the industry but also because those concerned feel that they have a considerable responsibility for the safety of the general public. No mechanic is recognized as fully trained in the industry unless he has taken a part-time motor mechanic's course at a technical college, has passed the appropriate Intermediate City and Guilds examination and has successfully passed a practical examination set by the industry itself. The difference between this industry and others is that it has not been strangled by tradition.

After reviewing the present system of recruiting and training young workers for skilled jobs, Lady Williams turns to the future. In all essential points, she writes, the present system is exactly the same as the method introduced in the twelfth century; the old system of training has been carried on without any question of its suitability for a profoundly different situation.

Lady Williams is to be warmly commended for carrying out this first detached investigation into methods used by Britain's industry for training its young workers; their continuation along present lines could easily bring the nation to bankruptcy. As she disturbingly points out, it is astonishing that we still accept a system whereby apprenticeships must last five years, must begin at 16 and end at 21; in which each craft needs five years to master it; in which a boy who does not decide to become a craftsman at the age of 16 may, in general, never become one.

That the British method of producing skilled workers is not the only way is shown by experience in other countries. In the United States, for example, formal apprenticeship must last at least two years, but the period varies from one industry to another and may last five years. The age at which apprenticeship must begin is equally flexible. In many of the States there is no upper age limit for entry; in some it is 24 with allowances for higher age limits for

ex-servicemen. Nor does an apprenticeship come to an end merely with the passage of time; in some trades an apprentice has to pass certain tests of competence before proceeding to further training or passing out as qualified.

What can be done by a determined nation has been shown by France. Before the Second World War the training of skilled workers was not in a very satisfactory state. The need for rebuilding a shattered economy exposed the extreme shortage of skilled workers; and as employers were afraid to commit themselves to a long period of training for young people, the technical section of the Ministry of Education undertook to provide a complete craft training in a full-time course lasting three years and covering a wide range of skills. The result has been to create a system of craft training which makes the British method one which the economy can no longer afford. The story is the same in Germany, Holland, Switzerland and other industrial countries.

Lady Williams's book should help to loosen some of the smugness and complacency which bedevils some sectors of British industry. It should be placed on the agenda for consideration of every board.

T. H. Hawkins

FOUR-DIMENSIONAL GAS

The Relativistic Gas

By Prof. J. L. Synge. (Series in Physics.) Pp. xi+108. (Amsterdam: North-Holland Publishing Company, 1957.) 15 guilders.

THIS book is valuable, both in itself and as an addendum to the author's recent "Relativity: the Special Theory". That excellent treatise was remarkable for its combination of breadth and single-mindedness. All was based on four-dimensional space-time as a geometrical entity; and we were skilfully persuaded that flat four-dimensional geometry is as natural for non-gravitational theory as curved space-time is for general relativity. We may have qualms, however, when now we meet a direct four-dimensional approach to gas theory; for when the equations are automatically Lorentz-invariant one hesitates to enclose the gas in a three-dimensional bottle. A Lorentz transformation would send the bottle flying. Thus we should be happier if we fixed the bottle in our own space-frame, and merely examined the statistical effect of the relativistic variation of the masses of the gas particles with their individual velocities. F. Jüttner in 1911 and R. C. Tolman in 1914 had treated the equilibrium distribution from this point of view. Jüttner, coming closer to Synge, had transformed the distribution integral into a modified Bessel function, and also had noted incidentally the relevance of four-dimensional momentum-space to his transformation.

Prof. Synge begins directly with space-time and four-vector momentum. The 'points' of space-time are 'events', and the 'lines' the histories of free particles. Thus we have in a single picture the history of a gas of particles displayed as a 'broken discrete complex'—line fragments concurring in fours in collision-events. (It is suggested that this picture supports the notion that point-particles manage to collide because they are 'fated' to.) The gas is *not* bottled, and attention is concentrated first on the momentum distribution near some general point-event A. This is calculated by assuming that the 4-momentum has the most probable distribution for an assigned total flux over an elementary null-cone at A. The energy tensor then serves to identify the pressure of the gas (as a perfect fluid), and by its space-time conservation laws to give the equations of motion. Temperature is identified primarily as a common parameter in a gas mixture; but more thermodynamics appears in the last chapter, which treats the theory of shock-waves. This is of great interest, and the four-dimensional picture here is perspicuous. The mathematics of the shock velocity and temperatures involves the modified Bessel functions characteristic of relativistic distributions, rather awkwardly; but the author (assisted here by Mr. H. F. Sandham) completely overcomes the obstacles.

A basic tool in the earlier part is the "absolute 2-content of a 3-cell" on the pseudo-sphere of momentum of a particle. This 2-content is, rather strangely, proportional to the product of three differentials (being dimensionally the ratio of a 3-volume element to a length), but has the vital property of retaining significance when the rest-mass tends to zero and the pseudo-sphere to the null-cone. This concept enables the author to transfer the whole theory to zero-mass particles, which may be described as Maxwell–Boltzmann photons; the results are, as he says, too attractively simple to leave out. This characteristic lightness of touch serves as a foil to the solid achievement represented by the book, which provides a strong stimulus towards relativistic procedures and modes of thought.

N. B. Slater

GUIDE TO ARCHÆOLOGY

Archæology and Society

Reconstructing the Prehistoric Past. By Prof. Grahame Clark. Third edition, revised. Pp. 272+24 plates. (London: Methuen and Co., Ltd., 1957.) 25*s*. net.

"ARCHÆOLOGY AND SOCIETY" was first published as long ago as 1939 and a second edition appeared in 1947. It is not surprising that ten years later a third edition has been called for. The first two-thirds of the volume are concerned with accounts of how discoveries can be made and what sort of direct evidence as to the life and outlook of our remote forerunners and ancestors can be deduced from them. This section has been entirely brought up to date and should be read and digested by every would-be prehistorian. The last third of the book attempts to probe deeper into the daily lives of prehistoric man. Unfortunately only certain materials can survive and only objects made of these materials can help us. Again, whether on the economic plane or on that of religion, some of the evidence that has survived can often be interpreted in many different ways. It is all too easy tacitly to assume that our modern outlook on things, or even that of modern primitive man, is the same as that of these prehistoric folk. It is all-important, therefore, that anyone who tries to tackle these problems should himself have the widest possible knowledge and, living as it were with them, should have almost come to see life through their eyes. No one could be found better qualified for this purpose than Dr. Grahame Clark, and his conclusions on these matters can, therefore, be accepted as very reasonable. The book is illustrated with line blocks and full page illustrations. It is very satisfactory that a third edition of this well-known work is now available.

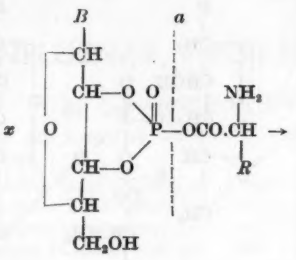
M. C. Burkitt

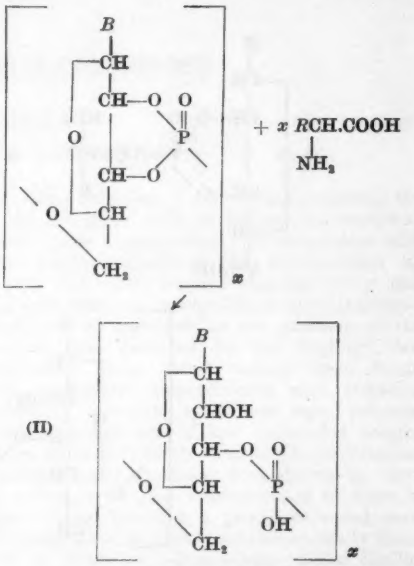
A HYPOTHESIS FOR THE BIOSYNTHESIS OF RIBONUCLEIC ACID AND PROTEIN

By Dr. A. M. MICHELSON

Arthur Guinness, Son and Co., Ltd., Chemist's Laboratory, St. James's Gate, Dublin

THE polymerization of nucleoside-2′ : 3′-cyclic phosphates by the action of acid anhydrides[1] is in some ways analogous to the enzymatic synthesis of di- and tri-nucleotides from pyrimidine cyclic phosphates by the action of ribonuclease[2] and may also have a bearing on the biosynthesis of both ribonucleic acid and protein. Amino-acid anhydrides of 5′-nucleotides have been described[3,4] and postulated as intermediates, as have non-identified precursors involving both an amino-acid and a nucleotide[5]. While it has been suggested[6] that polynucleotide formation occurs from the 5′-phosphate to a 3′-hydroxyl (as is the case in Ochoa's enzymatic polymerization of nucleoside pyrophosphates[7]) it is as feasible that the actual function of the 5′-nucleotide amino-acid derivative is to act as a specific anhydride on the nucleoside-2′ : 3′-cyclic phosphates to give intermediates which would react rapidly with other such intermediates to give ribonucleic acid and/or protein. Experimental evidence of such a transfer of amino-acid to a nucleotide factor is given in a recent communication by Webster[16].





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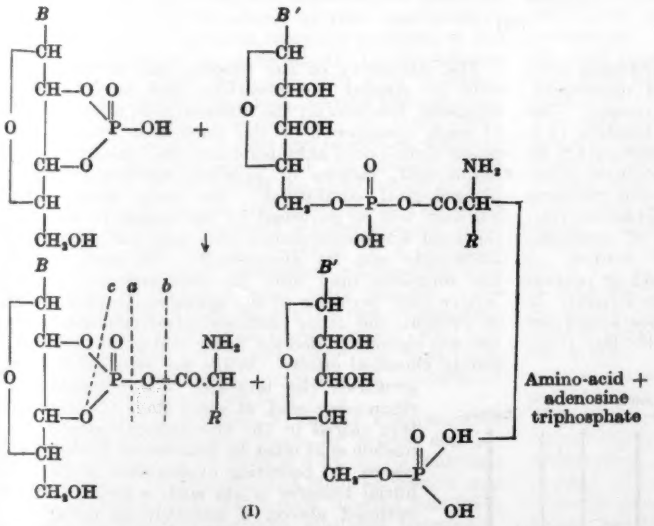


(I)

Amino-acid + adenosine triphosphate

Cleavage of this postulated intermediate could occur at each of the lines marked *a*, *b* and *c* (I). Fission at *a* or *b* is analogous to the acid or alkaline hydrolysis, respectively, of acetyl phosphate[8], while that at *c* is comparable with the hydrolysis at all *p*H's of dialkyl esters of uridine-3′-phosphate[9]. The consequences of such cleavage can be summarized as follows.

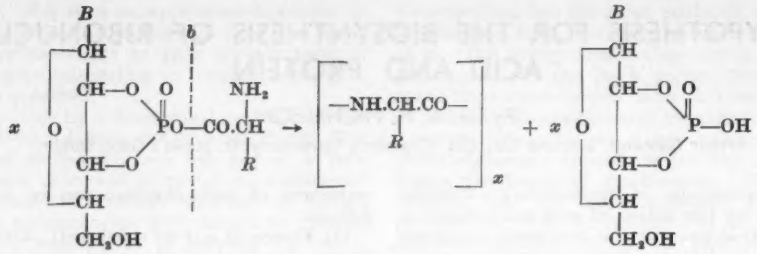
(1) Fission at *a* (acid catalysed). Attack at CH_2OH group. Synthesis of ribonucleic acid (reaction II).

(2) Fission at *b* (base catalysed). Attack at NH_2-group. Synthesis of protein (reaction III).

(3) Fission at *c*. Attack at CH_2OH group. Fission at *b*. Attack at NH_2-group. Synthesis of ribonucleic acid and protein (reaction IV).

Cleavage at *a* or *c* and attack at the amino-acid NH_2-groups would produce phosphoramidates, themselves reactive intermediates for mixed anhydrides[10,11], and can therefore be disregarded.

Earlier hypotheses[12,13] have postulated the transfer of amino-acid (activated through the carboxyl group) to a nucleic acid template to form an acyl anhydride with each internucleotide linkage, followed by formation of peptide bonds. With such hypotheses it is difficult to account for the inhibition of polynucleotide metabolism by inhibitors of protein synthesis which do not inhibit the incorporation of precursors into soluble nucleotides, that is, an effect concerned with the actual conversion of nucleotide into polynucleotide[14]; neither do they provide a ready explanation for the varying rates of protein and nucleic acid synthesis.



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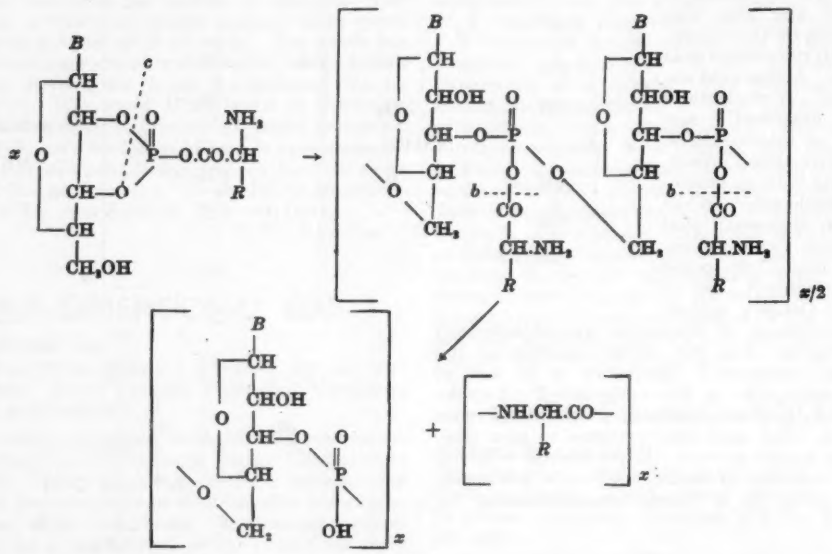
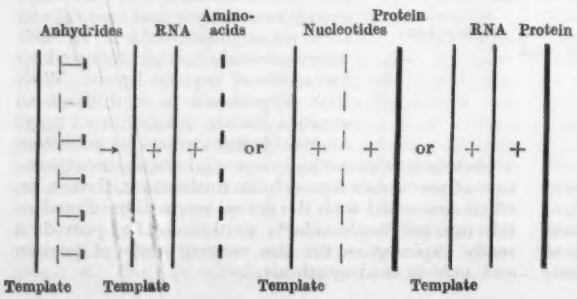


(IV)

It is suggested in the present hypothesis that biosynthesis occurs by the addition of nucleoside-2′ : 3′-cyclic phosphate–amino-acid anhydrides. The nucleotide bases are held by hydrogen bonding to a strand of nucleic acid in an order complementary to it, or to the enzyme itself. As each polymer (ribonucleic acid and/or protein) is formed the products peel away to assume their own characteristic configurations. Depending on the type of cleavage, directed enzymatically, possibly by control of effective local *p*H, either ribonucleic acid or protein or both ribonucleic acid and protein are formed. It is conceivable that small oligonucleotides would act as primers for such reactions, as with the polynucleotide phosphorylase of Ochoa[15].



The specificity of the process can be based not only on spatial considerations and bonding to a template, but also on the differences in acid strength of each component of the nucleoside cyclic phosphate–amino-acid anhydride and the basicity of each amino-acid, factors of possible significance under physiological conditions. In some degree each addition will be governed by the nature of both the terminal nucleotide–amino-acid and the succeeding nucleotide and its amino-acid. To some extent the template may only be necessary for limited 'active core' portions of the synthesized nucleic acid or protein, the other portions being restricted to a not too rigorously defined order and composition by purely chemical effects. While for cytoplasmic biosynthesis the template is the enzyme or ribonucleic acid, at some stage the information stored in the chromosomal deoxyribonucleic acid must be transferred to the cytoplasm. A tempting explanation is that the initial transfer is via such a mechanism as outlined above, a molecule of deoxyribonucleic acid or part of such a molecule being the template.

The essential part of this hypothesis accommodates most of the recorded biochemical data on the biosynthesis of peptides, proteins and nucleic acid[6,13] and is based on reasonable chemical analogies. It is

hoped to develop such chemical aspects as the synthesis of oligonucleotide-peptides and ribonucleoprotein.

Thanks are due to the directors of Arthur Guinness, Son and Co. (Dublin), Ltd., for permission to publish this communication.

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THE INFLUENZA VIRUS NEURAMINIDASE

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VIRUSES in their simplest form (tobacco mosaic virus, coliphages, polio virus) are nucleoproteins, the nucleic acid carrying the genetic information for the make-up of the virus generation to come, and the protein coat protecting the genetic material on its way from host cell to host cell. With increasing size the chemistry of the viruses becomes more complex. Irrespective of their composition, the viruses proper have no metabolism of their own. They integrate themselves into the metabolic pattern of their host cells. By imposing their code of sequence on the arrangement of the host building blocks in the enzymic synthesis of nucleic acids and proteins, they re-direct the chemical reactions to their own end[1]. The first serious challenge to the prevailing view that viruses are devoid of enzymes was the observation by Hirst[2] that chicken erythrocytes adsorbed influenza virus and by doing so were agglutinated. Whereas at 4° C. the virus remained attached to the red cells for 18 hr. or more, at 37° C. spontaneous elution of the virus from the cells soon occurred. The virus liberated was intact, leaving behind, however, the red cells inagglutinable by fresh virus. Hirst's interpretation, logical and ingenious, was that a receptor substance present at the surface of the red cell attracted and bound the virus, which in turn by virtue of an enzyme embedded in its coat acted upon the receptor. The viral enzyme-receptor interaction was envisaged as resulting in a chemical change of the receptor substance concomitant with the release of the unimpaired virus particle. The same sequence of events—adsorption and spontaneous elution of the virus—was demonstrated[3] with the system influenza virus and surviving host cells deprived of their normal blood supply (respiratory cells of the excised ferret lung).

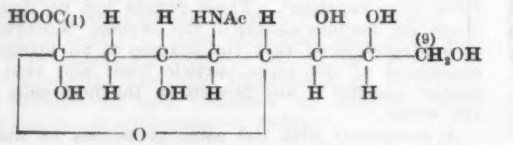
Much weight was added to Hirst's interpretation by subsequent discoveries made by Burnet and his co-workers[3]. They showed that a variety of materials secreted by mucus membranes such as gastric and intestinal mucin, bronchial mucus, ovarian cyst fluid, cervical mucus, etc., strongly inhibited hæmagglutination by indicator virus, that is, virus treated (by heat) in such a way as to prevent it being eluted spontaneously from erythrocytes. When digested with active virus, these inhibitory mucins lost their biological activity. Most useful and important was the preparation by Burnet *et al.*[3] from the culture filtrate of *Vibrio cholerae* of an enzyme imitating the effect of the influenza virus on the cellular receptors and the inhibitory mucoproteins. Pretreatment with the vibrio enzyme rendered the erythrocytes inagglutinable, the mucoproteins non-inhibitory and the intact host cells non-susceptible to virus infection.

The biochemical approach to the problem of the viral enzyme was initiated by the finding[4] that living influenza virus, when acting upon hæmagglutinin inhibitory mucoproteins and reducing their inhibitory capacity by 99 per cent, released from them a compound of low molecular weight separable by dialysis from the digest. On purification this compound (split product) was shown to have reducing power, to contain nitrogen and to form in the Morgan–Elson reaction a purple-coloured substance indistinguishable spectrophotometrically from that given by N-acetyl D-glucosamine under similar conditions[4]. However, in contrast to amino-sugars, the split product gave a direct Ehrlich reaction and a positive Bial's orcinol test, and it decomposed with formation of humin on treatment with mild acid[4,5]. The split product was the same whether a mucoprotein fraction of hen egg-white (ovomucin) or an electrophoretically homogeneous mucoprotein from human urine was taken as substrate, whether influenza virus or vibrio enzyme was used as catalytic agent. Appropriate controls with heat-inactivated virus were negative[4]. These results left no doubt about the enzymic nature of the process. Moreover, it was established that the enzyme is an intrinsic compound of the virus particle itself and that a similar enzyme is not present in the host cells of the virus[1].

A compound with the same properties as those described for the split product was first isolated from bovine submaxillary gland mucoprotein by Blix[6]. This compound, termed by Blix *et al.*[7] sialic acid, was eventually proved to have the composition $C_{11}H_{21}O_{10}N$ and to contain an N-acetyl group, an O-acetyl group which is easily split off, a carboxyl group, an α-keto or α-hydroxyl group, a primary alcohol group and a group titratable by hypoiodite[7]. Klenk and Lauenstein[8], treating bovine submaxillary gland mucoprotein and human urine with 5 per cent methanolic hydrochloric acid, obtained methoxyneuraminic acid, a substance resembling sialic acid in the reactions carried out at acid *p*H, but differing from

it in being non-reducing, alkali-stable, ninhydrin-positive, free of acetyl and in possessing a methoxy group. The close relationship between sialic acid and neuraminic acid became obvious when N-acetylneuraminic acid was isolated from the hydrolysate of bovine submaxillary gland mucoprotein[8]. Confirming the earlier observations on the release of the split product from human urine by virus action, Klenk *et al.*[9] crystallized the product and identified it as N-acetylneuraminic acid. It seemed that the latter differed from sialic acid only in the lack of an O-acetyl group. Therefore, N-acetylneuraminic acid and methoxyneuraminic acid would be expected to have the composition $C_{11}H_{19}O_9N$ and $C_{10}H_{19}O_9N$, respectively. Instead, Klenk *et al.*[8,9] assigned to them the formulæ $C_{13}H_{21}O_{10}N$ and $C_{11}H_{21}O_9N$.

The molecular structure of N-acetylneuraminic acid, exhibiting properties of both an amino-sugar and a 2-deoxyhexose, remained obscure until a well-defined degradation product was prepared. Bovine submaxillary gland mucoprotein and human urine, on treatment with alkali, yielded pyrrole-2-carboxylic acid, and the same pyrrole derivative was obtained from the split product[10]. On this basis and on the other information available, neuraminic acid was envisaged as an aldol-condensation product of D-glucosamine (or D-galactosamine) with pyruvic acid, methoxyneuraminic acid as the methylglycoside of neuraminic acid, N-acetylneuraminic acid as the N-acetylated neuraminic acid and sialic acid as ON-diacetylneuraminic acid[10]. This concept was strengthened by the synthesis[10] of pyrrole-2-carboxylic acid from D-glucosamine and pyruvate at *p*H 11·0 and was confirmed by the reverse aldolization of N-acetylneuraminic acid to N-acetylglucosamine and pyruvic acid[11] or pyruvic acid degradation products[12]. Its final proof was derived from the synthesis of N-acetylneuraminic acid from N-acetyl D-glucosamine and carboxylated pyruvic acid (oxaloacetic acid), the synthetic compound being chemically and stereochemically identical with the crystalline compound of biological origin[13]. By the method of synthesis the carbon skeleton, the nature and position of the functional groups are determined and Blix's formula proved to be correct. The stereochemistry at C_2 and C_4 of crystalline N-acetylneuraminic acid was recently elucidated[14]. The pyranose structure and the position at C_7 of the O-acetyl in diacetylneuraminic acid follow from the results of periodate oxidation[7].



N-Acetylneuraminic acid

It was early recognized that the inhibitory mucoproteins are conjugated proteins containing as prosthetic groups polysaccharides or oligosaccharides of small size distributed in numbers along the polypeptide chain[1]. Sialic acid, in the new terminology[15] the group name for all acylated neuraminic acids, occupies a terminal position in the prosthetic groups and is linked through its potential keto-group glycosidically to the adjacent sugar unit[16]. Bovine submaxillary gland mucoprotein contains about 17 per cent diacetylneuraminic acid and an equimolar amount of N-acetylgalactosamine besides very small quantities of N-acetylglucosamine, galactose, mannose and fucose. At *p*H 1·0 and 80° C., 78 per cent sialic acid—and sialic acid only—is released; vibrio enzyme splits off 64 per cent of the sialic acid present in bovine submaxillary gland mucoprotein. The prosthetic group may be detached by very dilute alkali. It has been obtained as a white substance in an analytically pure state; its formula is $C_{19}H_{32}O_{14}N_2$ and it consists of N-acetylneuraminic acid linked ketosidically to N-acetylgalactosamine. The viral enzyme and vibrio enzyme split the disaccharide into N-acetylneuraminic acid and N-acetylgalactosamine (Gottschalk, A., and Graham, B., in preparation). The ready conversion even at room temperature of the disaccharide at *p*H 10 and at *p*H 2 to chromogen-disaccharides, that is, disaccharides the N-acetylgalactosamine moiety of which is transformed to a chromogen in the direct Ehrlich reaction, would suggest linkage of N-acetylneuraminic acid to C_6 of the amino-sugar (in its furanose form), though linkage to C_6 cannot be excluded. A furanose structure of N-acetylgalactosamine within the disaccharide would also account for the direct Ehrlich reaction of the latter in the cold and for the ease with which N-acetylneuraminic acid is released from bovine submaxillary gland mucoprotein at its own *p*H. These new findings may necessitate re-interpretation of previous observations on the prosthetic group[16].

The specificity of the virus enzyme and vibrio enzyme is not absolute. Thus the above disaccharide, three chromogen-disaccharides and sialyl-lactose, a reducing trisaccharide consisting of diacetylneuraminic acid, linked α-ketosidically to lactose[14], are readily hydrolysed by the enzyme. N-glycolyneuraminic acid, the sialic acid of porcine submaxillary gland mucoprotein, is split off from this mucoid by vibrio enzyme[9]. The influenza virus enzyme and the isodynamic vibrio enzyme may, therefore, be defined as neuraminidases cleaving the α-ketosidic linkage joining the terminal acylated neuraminic acid to another sugar or sugar derivative.

Finally, it may be mentioned that N-acetylneuraminic acid has been obtained crystalline from human erythrocytes by the action of vibrio neuraminidase[9]. This achievement suggests that the cellular receptors are sialylmucoproteins like their chemical analogues, the hæmagglutinin inhibitory mucoids. The sialic acid component of both the cellular and the soluble receptors is instrumental in attracting and binding the virus, and the release of sialic acid from both types of receptors by the action of neuraminidase results in the loss of their biological activity.

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THE SPECIES: YESTERDAY AND TO-MORROW

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W. H. CAMP[1] has given powerful voice to the view that the species and other categories below the rank of family recognized in the codes of zoological and botanical nomenclature may well prove unserviceable as an instrument to describe the rich complexity of the units revealed by modern studies in micro-evolution and experimental taxonomy. "Our present system of nomenclature," he wrote, "was designed to fit a concept of static genera and species. Tinker with it as we will, we cannot re-tailor this mouldy shroud into something which will serve as an adequate nomenclatural covering for the complex, living groups which we are now beginning to realize must be defined. No one knows exactly what this system will be; as yet, one can see only a general outline, for our knowledge is too scant to permit a filling in of all the details."

In a communication to the British Association at Edinburgh in 1951[2] I strongly supported this view, and in a more recent paper[3], Prof. J. Heslop Harrison and I have put forward a tentative scheme, based on the suffix 'deme', for a new system of categories to express the findings of experimental taxonomy. In this last paper, the position of the species in relation to the deme terminology was briefly discussed. We stated that, in constructing our terminology, we had "rigidly excluded the categories of orthodox taxonomy", but we emphasized this did not mean that "we regard the terms genus, species, etc., as in any way outmoded. On the contrary, they will continue to remain essential as expressing the basic outline of living things and as a framework into which other classifications can be fitted". This view was further developed by both of us at a conference organized by the Botanical Society of the British Isles in 1954[4].

In the present article, I want to discuss more fully this question of the position of the species in relation to a system of categories for experimental taxonomy, and to put forward suggestions as to the future role of the species concept in biological taxonomy.

The term 'species' was first used more or less in its modern sense during the seventeenth century, and John Ray, in 1674, was among the earliest to lay down criteria for the assessment of specific differences. During succeeding years the concept gradually became stabilized to denote the basic units of biological classifications—the units, in fact, on which classification operated. These units were regarded by the great majority of biologists as static entities, created once and for all; the task of the systematist was to look for them, to recognize them for what they were, and to group them into higher categories. John Lindley, who took a more naturalistic view of plant classification than many of his contemporaries and who described "genera, orders, classes, and the like, as mere contrivances to facilitate the arrangement of our ideas with regard to species", looked upon species themselves as "created by Nature", and this view, with varying degrees of theological adornment, may be taken as typical of the pre-Darwinian systematic biologists. The idea of species as discrete, specially created, entities was no doubt unconsciously reinforced by binary nomenclature with its superficial resemblance to personal human names like John Smith, and, by the middle of the nineteenth century, the species had become unshakeably enthroned in the centre of taxonomic thought.

With the general acceptance of the theory of evolution during the second half of the nineteenth century the history of the species concept entered a new phase—a phase from which it has not yet emerged. In view of its firm entrenchment, it was inevitable that the term species should have become at once accepted for the basic unit of evolutionary change—an acceptance that was, of course, of great value during the formative years of evolutionary theory and without which the theory could scarcely have developed.

It was not long, however, before it became clear that the species of pre-Darwinian taxonomists represented a number of very different types of unit from an evolutionary point of view. The existence of such different types had, of course, long been recognized, and the 'lumpers' and 'splitters' represented two extreme reactions to this fact; but it was not until the acceptance of evolution, and especially the development of genetics and cytology during the past fifty years, that the reasons for these differences began to be appreciated.

The reaction to this appreciation took the form of attempts both to define the species in modern evolutionary terms, and to set up new categories to supplement or replace it. With regard to the former, there has been a growing realization that, owing to the disparate nature of the units usually classed as species, it is not practicable to choose an evolutionary definition that is of use in more than a very limited sphere. For example, a definition based on intersterility leads, in some cases, to the separation of individuals practically indistinguishable morphologically. Such a definition clearly renders the species concept virtually useless to general biologists and others requiring an easily applicable and stable basic unit for taxonomy. These difficulties led to the second type of reaction, namely, the construction of special categories to express the new knowledge that was being accumulated. Such categories took the form either of modifications of the term 'species' (for example, Turesson's "ecospecies" and "coenospecies"), or of completely new terms (for example, Danser's "commiscuum", "comparium", etc.), but in either case their relationship to the species itself was far from clear and has led to much subsequent controversy.

In our paper on the deme terminology[3], Heslop Harrison and I gave reasons, based on the nature and purpose of classification in general, for favouring a complete separation between the categories of nomenclatural taxonomy and those of experimental taxonomy. This course has, I think, very definite advantages for both of the resulting category systems. With regard to experimental taxonomy, there is no doubt that the term 'species', however conscientiously one tries to forget its past associations and to adapt it to modern ideas, still comes to us trailing clouds of special-creation glory, which, as Camp has in-

dicated, tend to bedevil its use in an evolutionary setting. It is largely these wisps from the past that have caused so much controversy over the relationship between nomenclatural and experimental taxonomy. The problem of whether to adapt or to abandon a particular term is a familiar one in the history of thought, and which course should be taken depends on the adaptability of the term and the extent of the change that has taken place. For example, the humours of medieval medicine developed quite happily into the present-day bad and good humour of our dispositions, whereas phlogiston, which could not be adapted to modern science, is dead—and the word 'dryads' is not now employed to explain the behaviour of *Quercus robur*. On balance, the abandonment of the term 'species' as a category to express the findings of experimental taxonomy, reflecting a clean break with the non-evolutionary past, would seem to be the right policy. The reluctance of geneticists and others to take this step is, perhaps, bound up with a lingering belief that species are, in some way, 'real entities', not 'artificial constructs of the human mind' as are other taxa. This belief makes it seem unthinkable to abandon the term for such an important branch of biology as the study of micro-evolution. If, however, 'real entities' and 'artificial constructs' are regarded, not as mutually exclusive, but as alternative descriptions, each valid in its own context, this difficulty disappears. Such an approach has recently been fully developed by R. Crawshay-Williams in a most stimulating book[5].

The policy I am advocating is strongly reinforced by a consideration of the continued use of the term in nomenclatural taxonomy. Clearly, there will always be a need for a general-purpose classification, based primarily on morphology, for those who require a broad map of the diversity of living things. For this purpose, the nomenclatural categories of genus, species, variety, etc., are excellently suited, and it would be a great advantage if they were not subject to continued attempts to bring them up to date and to re-define them in evolutionary terms. Let them be regarded, frankly, as mainly morphological categories based on degree of difference and they will perform their proper function in biological classification. Such a view would lead to a reduction in the number of name changes made for taxonomic, as opposed to nomenclatural, reasons. For if it was agreed that genera and species should be regarded as broad, flexible categories, with a considerable element in them of convenience, biologists would not be so constrained to alter the limits of existing taxa in order to fit them to a particular taxonomic theory. This policy will, I know, strike many biologists as lessening the stature and importance of nomenclatural taxonomy. This is true only in the sense that, at the specific, or in some cases the generic, level detailed micro-evolutionary knowledge, when available, would be embodied in special categories, leaving the nomenclatural categories free to give a general purpose account of the variation of the organisms concerned. This does not mean, however, that attributes such as intersterility and chromosome number would be rigidly excluded as criteria for the nomenclatural categories; if they can be incorporated as supplementary to morphology without destroying the general utility of the classification the additional information thus embodied would be, of course, a valuable asset.

The suggestions outlined in this communication are an attempt to crystallize into a definite policy some of the rather ill-defined ideas that are in the air on the defects of present-day taxonomic theory and practice. They are put forward in the hope that others will be stimulated to think about them and discuss them, too, with the view of evolving a generally agreed policy.

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PHYSICS OF WATER AND ICE

THE discussion on "The Physics of Water and Ice", held at the Royal Society on November 14, was particularly timely. In the past two years there has been a renewed interest in both the experimental and theoretical aspects of ice and water in relation to their physical properties and their structure. On the theoretical side it has been evident for some time that the picture of ice and water as essentially regular and irregular arrangements, respectively, of water molecules tied together tetrahedrally by hydrogen-bonds is only a rough first approximation. Certainly it is inadequate for the degree of detail now required to define the properties of these substances. It is especially true for the properties that are so-called 'structure sensitive', that is, that depend on the variation of the crystalline material from its ideal state. At the same time, the practical interest in ice and water has been increasing from a number of fields, notably from glaciology, from the technical aspects of the icing of aircraft and aircraft engines, and perhaps most of all from meteorology, where it is now evident that the processes of conversion of water-drops into ice is fundamental for the phenomena of rain and thunderstorms. In the latter connexion it is interesting that a new type of interdepartmental co-operative research has now been going for three years at the Imperial College of Science and Technology, London, some of the main results of which were reported at the meeting. The meeting was not primarily concerned with the theoretical aspects of the hydrogen-bond, which had been recently covered in a discussion of the Section of Physical Chemistry of the International Union of Chemistry, at Ljubljana; nor with that of aqueous solutions, also recently dealt with at the Faraday Society meeting at Oxford. However, some of the leading Continental experts attending both those conferences also took part in the Royal Society discussion, and gave more details of their work in the field of pure water and ice.

Underlying all studies of ice is the knowledge of its structure at different temperatures. A critical survey of the data now available was given by Dame Kathleen Lonsdale (University College, London) in the opening paper, in which she discussed the results of the latest X-ray, electron and neutron diffraction experiments,

both for ordinary and for heavy ice. Both substances show some degree of differential contraction along the *a*- and *c*-axes on cooling, tending to make all the intermolecular distances of the water molecules in ice more nearly equal at the lower temperatures. This result, however, seems to contradict that reported by Dr. R. W. Powell, of the National Physical Laboratory, on the basis of careful interferometric measurements of the coefficient of expansion of large single crystals of ice in two directions. Such discrepancies, however, are not unknown in other instances, and there seems to be a systematic difference between X-ray and macroscopic measurements of thermal expansion, possibly due to crystal imperfections. However, the work ought to be repeated by both methods, preferably on the same specimen.

The neutron-diffraction work of H. A. Levy, of which a report was heard at the Discussion, indicates an almost completely random position for the protons along the hydrogen-bonds, corresponding to the picture given by Pauling, but Dame Kathleen Lonsdale pointed out that slight deviations from randomness could not easily be detected by this method.

Dr. G. B. B. M. Sutherland reported studies on the infra-red absorption of single crystals of ice, which he had carried out in conjunction with Dr. Ockmann. Over most of the frequencies concerned there seem to be very little preferred orientation and this would also favour an almost isotropic random hydrogen-bond structure for ice.

Prof. W. C. Price (King's College, London) reported some infra-red absorption studies of water adsorbed on alkali halides. Observed shifts in the frequencies of the 3,000-cm.$^{-1}$ band were interpreted in terms of water molecules attached by hydrogen-bonds to the negative ions. With thicker layers of adsorbed water, bands appear in the 2,000-cm.$^{-1}$ region, corresponding to molecular deformation plus a libration of the molecule against the forces of association with other molecules. Body-centred alkali halides show additional bands which suggest attachment of water to positive ions.

Dr. G. J. C. Frohnsdorff and Dr. G. L. Kington, working in Prof. R. M. Barrer's department in the Imperial College, reported some interesting thermodynamic and spectroscopic properties of water contained in the pores of synthetic zeolites, where up to twenty-one molecules of water can be accommodated. Here a transition can be observed between a predominantly adsorbed pattern on slight hydration, to a water-like pattern occurring close to saturation.

An important paper was read by Dr. H. Gränicher, reporting the studies of the Zurich school on the relaxation process, particularly dielectric relaxation in ice. Dr. Gränicher attributes part only of these phenomena to the presence of hydrogen ions in the form of oxonium and hydroxyl ions. He also postulates the presence of what he calls *L*- and *D*-protonic defects in ice in which the oxygen atoms are only slightly displaced. *L*-defects consist of two neighbouring oxygen atoms between which there is no proton; in *D*-defects two protons are more or less collinearly arranged between oxygen atoms. These types of defects are assumed to wander through the crystal structure, to be created and to neutralize each other. In this way good agreement is achieved with the new experimental results on the variation with frequency of the dielectric constant of ice at different temperatures. A theoretical treatment of lattice defects in relation to the mechanical relaxation of ice was given by Dr. R. Bass of Stuttgart.

The work of the Göttingen school was presented by Dr. L. De Mayer—Dr. M. Eigen, who is largely responsible for it, being unfortunately too ill to attend. It has long been known that the electrical conductivities of pure water and pure ice are of the same order of magnitude, and it is assumed that this is because the mechanisms operating in the two cases are similar. The Göttingen workers have managed to measure separately the number of ionic carriers, namely $(OH_3)^+$ and $(OH)^-$ groups, and their apparent velocities. They find that the number in ice is only a small fraction of that in water, and consequently the apparent velocity of hydrogen ions in ice must be many times as great as in water. As the velocity of hydrogen ions in water is already much higher than that of any other ion, and is clearly due, as Fowler and Bernal showed twenty-five years ago, by some form of relay mechanism, proton-transfer in ice must be by a similar mechanism, operating not over one hydrogen-bond but over a chain of hydrogen-bonds. This is very similar to electron-transfer in metals, and a close analogy exists between the two phenomena. It is even possible, by the introduction of fluorine ions, to produce what are equivalent to *p*- and *n*-junctions for ice, with protons taking the role of electrons in semiconductors. This work obviously opens up a very exciting field of theory which may have particular application in biology, following some recent suggestions by Szent-Györgyi.

Prof. H. S. Frank (United States) discussed the significance of some of the results already reported in terms of the nature of the hydrogen-bonds in water and ice, pointing out that the influence of the bond itself on the lone-pair electrons on the other side of the molecule leads to a general strengthening of the bond, and provides some explanation for the closer approximation of the properties along the *a*- and *c*-axes at low temperatures. This also goes some way to explain the very peculiar fact that the cubic form of ice, which has a diamond structure at first sight essentially similar to the wurzite structure of hexagonal ice, is not stable above − 110° C.

Turning now to the structure of water, Dr. C. L. van Pantaleon van Eck reported some very careful work recently carried out at Amsterdam on the X-ray diffraction patterns of water and on its infra-red absorption. His conclusions are that in the water-structure there are approximately six molecular neighbours at distances varying from 3·0 to 3·4 A. from any given molecule. It is not clear why this result is different from that obtained by Warren and Morgan in 1938, who found four molecular neighbours at 2·85 A. Dr. van Eck's interpretation would imply a collapsed tetrahedral structure not unlike that of metallic tin in relation to grey tin. He bases an independent calculation of the intermolecular distance in water on the absorption spectrum of HDO, in which the band at 3,500 cm.$^{-1}$, unlike the corresponding bands for ordinary and heavy water, is free from overlapping and permits an estimate leading to the value 2·85 A., as opposed to the hydrogen-bond distance of 2·76 A. found for ice. If this is the case then the hydrogen-bond in water would be considerably weaker than that in ice, of the order of 5 instead of 6 kcal./bond mole, and it would not be necessary to postulate any actual breaking of hydrogen-bonds on melting. These results throw open once more the whole question of the structure of water, and clearly much more experiment and calculation are necessary.

Dealing with the transitions from water to ice, and from water-vapour to ice, two papers were read by Dr. B. J. Mason, representing the work of the Imperial College group. In the first Dr. Mason gave a full acount of the extremely beautiful method which he has developed for growing small crystals of ice at controlled temperatures and water-vapour concentrations in a thermal-gradient air box. He has shown that temperature alone determines the particular habit adopted by ice crystals, except at high supersaturation when dendritic growth occurs. In the temperature ranges from 0° to − 3° C., from − 8° to − 12° C. and from − 16° to − 25° C., hexagonal plates are the rule. On the other hand, from − 3° to − 8° C., and below − 25° C., the predominant form is either the elongated hexagonal prism or the hollow prism which sometimes has a spiral structure. The fern-like dendritic crystals occur only between −12° and − 16° C. By growing crystals successively at different temperatures Dr. Mason has succeeded in producing combined forms which reproduce many of the beautiful effects long known in snowflakes, which may be interpreted as indicating the various temperature regimes through which the snowflakes moved during their formation. These observations are of fundamental meteorological importance and also of great physical interest, because until now very little work has been done on the influence of temperature on crystal-form, and it would appear that the sharp changes of habit with temperature indicate that we are dealing here with a thermodynamic phenomenon depending on the relative values of the free energy of growth in different habits, as Prof. J. D. Bernal suggested at the meeting.

Dr. Mason's other paper described a series of carefully controlled experiments, on the supercooling and freezing of water drops. He showed two curves relating the volumes of drops to the temperatures at which they froze—one for freezing induced by nuclei, and another for the case in which nuclei are absent. In the latter case supercooling for drops as small as 1μ in diameter can be effected down to − 41° C. and for 1-mm. drops down to − 33° C. Crystallization at these temperatures is an almost explosive process, resulting in the fracture of the frozen droplet and its shattering, a phenomenon which may be of great importance for the rapid spread of nucleation through super-cooled clouds in the atmosphere.

Dr. M. Blackman (Imperial College, London) reported a study by electron diffraction of epitaxial growth of ice on silver iodide crystals. Silver iodide is the only substance at present known to be capable of producing rapid nucleation of ice. By controlled growth of single crystals of ice, Dr. Blackman has been able to show the crystal formation of the ice with sufficient penetration to bring out the underlying silver iodide lattice.

J. D. Bernal

MIDDLE EAST TECHNICAL UNIVERSITY, ANKARA

By Dr. TURGUT YAZICIOGLU

THE foundation stone of the Middle East Technical University in Ankara was laid on October 3, 1957, in the presence of the President of the Turkish Republic, the Prime Minister, the Minister of Education, the heads of the foreign aid committees, the Ambassadors of Great Britain and of the United States of America and other distinguished guests.

The University has, in fact, been working temporarily in a building in Ankara for two years. In 1956 there were 175 students attending the Colleges of Architecture and Administration, and in February 1957 the College of Engineering was established, followed in September by the creation of a research department for nuclear physics.

Foundations for colleges of mining, geology, agriculture and forestry were laid in 1956, and in 1958 students of electrical engineering, mining and geology will be admitted.

There are at present five foreign professors at the University, but arrangements have been made for further staff to be recruited through the United Nations (including two professors of architecture and one of administration), through Unesco (including professors of construction engineering, and of machinery), and through the technical aid programme of the British Government (professors of physics and of electrical engineering).

Thus by this year there will be altogether twelve foreign professors on the University staff; the Turkish teaching staff will number thirty.

The site designated for the construction of the Middle East Technical University is about three miles from Ankara and covers an area of about 4,500 hectares (11,115 acres). The University campus will comprise, besides the college buildings, dormitories, faculty housing, libraries, a theatre, sports grounds, restaurants and other accommodation. All the plans and projects have been drawn up by the Turkish Ministry of Works in co-operation with the University of Pennsylvania.

According to the first ten-year programme, the University will have about 6,000 students, 2,500 teaching and research staff, and forty departments (including teaching and research units) at the end of the first decade.

During this ten-year period the Turkish Government is to make grants to the University totalling 265 million Turkish lira (about £33 million), of which almost half will be invested, while over the same period foreign aid will be available to a total of 26·5 million dollars. In addition, the British Ambassador, in his speech during the inaugural ceremony, promised a further sum of £25,000 from the British Government in the present year.

The University is designed to fulfil the need for an international centre of higher learning to serve the requirements of the developing Middle East. It is founded on the belief that the development of the region in a manner satisfying the demands of a progressive society requires the integration of techniques with human needs and aspirations. The University considers its major responsibility to be the training of a growing number of educated men and women, able to work creatively on the problems of the Middle East, using the most advanced techniques of our era.

History of the University

Constituted as a university only since January 13, 1957, the Middle East Technical University now has two faculties. From the single group of fifty students, with which the teaching programme opened in November 1956, the Government of Turkey plans that the University shall grow to major size, involving six faculties, with a complete research establishment matching the academic activities of each department with a programme of applied research and development work in industrial and agricultural problems. The University's teaching programme, conducted in English, with its emphasis on unity of thought and action, and on the correlation of teaching and research, should make it an institution unique in the Middle East.

The first step in the establishment of the University was taken in 1953, when a survey of building and planning legislation revealed the need for university-trained technical people in the professions. As a result of this survey, a school of architecture and town-planning was suggested, to be integrated with research institutes in building and planning. A complete programme for the school was prepared by a United Nations Mission, under the direction of Dr. G. Holmes Perkins, dean of the University of Pennsylvania School of Fine Arts.

The Perkins Committee received not only the enthusiastic and energetic support of the President of Turkey, the President of the Grand National Assembly and the Prime Minister, but also the effective co-operation of a committee for the school, headed by the Minister of Education, and having as members Mr. Vecdi Diker, Mr. Ahmed Tokus, Mr. Mihat Yenen and Mr. Adli Yener.

Following the Perkins Committee recommendations, a request was submitted to the United Nations for assistance in establishing the first faculty. In the autumn of 1956 the Committee was reactivated by the Minister of Education as an acting Board of Trustees. Under its guidance, the programme of the University has been developed and its teaching operations begun. Fifty students first received instruction in architecture on November 1, 1956, in a rented building at 18 Müdafaa Caddesi, in Ankara. The formal inauguration ceremony was held on November 15.

During the winter, plans were made for expansion of the teaching facilities and the acquisition of a campus. On January 13, 1957, a provisional law authorizing the establishment of the University was passed by the Grand National Assembly. The Minister of Education has set aside a magnificent site for the development of a teaching and research centre, and plans for the construction of University facilities are moving ahead at top speed.

Since the beginning of the spring term 1957 the second faculty, that of engineering, has been in operation. Its first Department, of Mechanical Engineering, was joined by a Department of Civil Engineering last autumn. At the same time, the faculty of administration was opened. Continuing to develop in this way, all six faculties with their research institutes will, it is hoped, be in existence within five years, permitting rapid expansion toward the goal set by the Government.

OBITUARY

Viscount Waverley, G.C.B., O.M., G.C.S.I., G.C.I.E., F.R.S.

THERE have been many tributes to the outstanding qualities of mind and character which made John Anderson, the first Viscount Waverley, one of the greatest, beyond all question, of the many great public servants of Great Britain. It may be doubted, however, whether any of the numerous accounts of his remarkable career have yet done full justice to the special nature of the service which he was qualified to render to the nation, especially in two world wars, by his experience as a student of science at Edinburgh and a postgraduate research worker at Leipzig, before he entered the Civil Service. It should be understood that this course of scientific studies and practice in research did not serve him as a mere introduction, or a stepping-stone, to an official and ministerial career of such extraordinary distinction, in which he became a permanent Under-Secretary of State (Home Office) by the time that he was forty, governor of Bengal ten years later, and then, as Member of Parliament for the Scottish Universities, Minister for Home Affairs and Home Security, Lord President of the Council and, finally, Chancellor of the Exchequer, during the Second World War. Through it all, and with his imperishable and comprehensive memory as a background, the supreme importance of scientific progress had remained with him a matter of central interest and conviction. It was no mere matter of curious coincidence that the subject for his postgraduate researches and Ph.D. thesis at Leipzig had been the chemistry of uranium, and that nearly forty years later, and behind the densest curtain of war-time secrecy, he had found himself charged with the chief responsibility, on behalf of the War Cabinet, for Britain's part in the development of researches on atomic energy—primarily, of course, for purposes of war, but with an ultimate view, even then, to peaceful applications.

My first encounter with the future Lord Waverley was soon after the outbreak of the First World War, in 1914. Sir Robert Morant, as chairman of the English Health Insurance Commission, had found himself suddenly faced with the abrupt disappearance of all the important drugs which had been obtained from Germany; and he promptly recognized that in John Anderson, a brilliant young Civil servant, with the then almost unique qualification of scientific training and experience, he had the man to deal with the position thus created. I had newly joined the scientific staff of the then embryonic Medical Research Committee (now Council), and its first secretary, Walter Fletcher, had asked me to act on its behalf with reference to the same emergency; so that Anderson and I naturally met and kept contact over problems of common interest and responsibility. He also became a member, and a most important one, of a Royal Society Committee, appointed to enlist scientific co-operation to meet sudden needs of the same kind. Thereafter he was swept away to heights of administrative promotion where I lost

direct touch with him; and it was not, indeed, until the Second World War that I came again into even closer scientific and personal contact with him, and then mostly 'behind the scenes'.

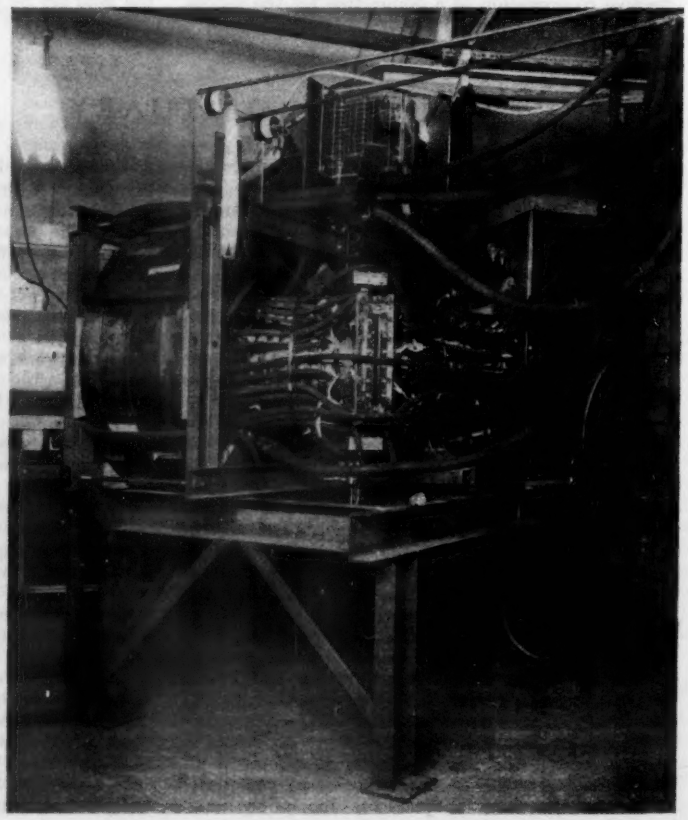
When the university memberships were abolished, Sir John Anderson, having no real interest in party politics, retired from parliamentary life until he entered the House of Lords. He became chairman for his remaining years of the Port of London Authority; and he continued to give practical evidence of his interest in scientific matters, not only as a director of Imperial Chemical Industries, Ltd., but also through a variety of honorary services to science, such as those of chairman of the Parliamentary and Scientific Committee, treasurer of the Lister Institute and president of the Research Defence Society. Nobody who was privileged to work closely with him could have failed, I think, to become aware that, behind the mask of his almost legendary reserve and quiet assumption of authority, Lord Waverley retained a generous and even emotional attachment to science and its claims, though an open display of such an enthusiasm was scarcely within his range.

H. H. Dale

NEWS and VIEWS

SCEPTRE III

Following the article by Allen, Allibone *et al.* in *Nature* of January 25, p. 222, we publish a photograph of SCEPTRE III. The photograph shows part of the 4-ton strip steel core threading the torus. Each half of the core has an eight-turn winding carrying 50 amp. d.c. to provide flux biasing. The primary, consisting of four separate windings of eight turns each, is wound as uniformly as possible over the surface of the torus. The torus has a bore of 12 in. and is made of $\frac{1}{4}$-in. thick aluminium in four quadrants, or sectors, separated by four straight sections each 6 in. long, which provide viewing and pumping facilities. The eight insulating gaps are protected by eight pairs of interlaced copper liners, and $\frac{3}{8}$ double vacuum gaskets made from indium wire are used throughout. Quartz-covered slits—one in the vertical plane and one horizontal—allow the discharge to be viewed by a high-speed rotating mirror camera. The primary windings are arranged to obscure these slits as little as possible. The toroidal magnetic field is created by providing a d.c. supply to 102 layer-wound coils placed around the torus perimeter beneath the primary winding, and at pumping and viewing ports the field is maintained constant by compensating coils. At the maximum B_z field of 1,000 gauss some 125 kW. are dissipated, and since these coils are not water-cooled the time of operation of the apparatus is limited by the temperature rise of these coils. The capacitors can be discharged into SCEPTRE III 40 times a minute, and about 100,000 discharges have been produced without noticeable deterioration of the torus lining. The terminating-circuit of the radio-frequency pre-ionizing supply is seen mounted above the core, and the water-cooling feed lines and a lead chamber containing the nuclear detectors are on the right.



Dr. J. R. Killian, jun.

As special assistant for science and technology to the President of the United States, Dr. James Killian brings to that post, specially created for him last November, a good deal of scientific and administrative experience. The position was described by President Eisenhower, in his address on "Science and National Security" on November 7, as one in which the incumbent, "aided by a staff of scientists and a strong advisory group of outstanding experts . . .", would "have the active responsibility of helping me

follow through on the (scientific and development) programme". Thirty years ago, at the age of twenty-three, Dr. Killian took his degree of bachelor of science in business and engineering administration. The course for that degree covered many aspects of engineering and science and also comprised training in engineering and scientific management. Since then, his work in research and administration has won for him many honours and awards, including the President's Certificate of Merit in 1948, the Freedoms Foundation Award in 1952 and a Certificate of Appreciation from the Department of the Army in 1953.

Dr. Killian is a Fellow of the American Academy of Arts and Sciences and a member and former vice-president of the American Society for Engineering Education. In 1951, he was appointed chairman of the Army Scientific Advisory Panel and a member of the Scientific Advisory Committee of the Office of Defense Mobilization. Dr. Killian went to the Massachusetts Institute of Technology from Trinity College (now Duke University) in 1923, and while he was editing its newspaper, *Technology Review*, was made executive assistant to the Institute's president, the late Dr. Karl T. Compton. Two years after this appointment, in 1939, he was helping Dr. Compton to organize the Institute's vast programme of training and research for war purposes; ten years later, Dr. Killian succeeded Dr. Compton as president of the Institute.

College of Aeronautics, Cranfield : Prof. John Loxham

Mr. John Loxham has been appointed to the chair of aircraft economics and production in the College of Aeronautics, Cranfield. He will take up his appointment on April 1. Mr. Loxham, as managing director of the Sigma Instrument Co., Ltd., has been responsible for the development of a wide range of fine measuring equipment specially suited to the production methods of modern industry, and those of the aero-engine industry in particular. His work in this field has earned him an international reputation, and machines made to his designs are used in all the industrialized countries of the world. Before joining the Sigma Company in 1939, Mr. Loxham was head of the Engineering Production Department at the Northampton Polytechnic, London, where he established the first National Certificate course in production engineering, and set up the first technical college machine tool laboratory. He is chairman of the British Standards Institution Committee for Limits and Fits, and a member of several other committees. He has lectured in Great Britain and abroad on subjects associated with management, technical education and the control of quality in engineering production. In 1946, he was awarded the Sir Joseph Whitworth Prize by the Institution of Mechanical Engineers for his paper on "An Experiment in the use of a Standard Limit System", and the City and Guilds of London Institute has recently conferred upon him the Insignia Award in Mechanical Engineering for 1957 in recognition of his work in the field of precision engineering.

Horticulture at Reading : Prof. O. V. S. Heath

Dr. O. V. S. Heath, who has been appointed to the chair of horticulture in the University of Reading, graduated in botany in 1925 at the Imperial College of Science and Technology, London, and proceeded to the Imperial College of Tropical Agriculture, Trinidad, for a course in tropical crop botany. During 1927–36 he held the post of plant physiologist at the Cotton Experiment Station, Barbeton, South Africa, and carried out developmental studies on cotton in relation to soil moisture, rotations, and cultivation methods. In 1936 he returned to the Imperial College as Leverhulme Research Fellow and began work on carbon dioxide assimilation in relation to stomatal aperture, and the mechanism of stomatal movement which from then on became his major field of work. In 1939, he was appointed to the staff of the Research Institute of Plant Physiology at the Imperial College and attained the rank of senior principal scientific officer. During 1940–45 he carried out research on morphogenesis in the onion plant, especially the effects of day-length and temperature on bulb formation and flowering. Techniques for producing onion sets and for preventing bolting by storage treatment were devised, and low-bolting strains selected. From 1945 onwards Dr. Heath resumed his stomatal investigations. The discovery of the effects of carbon dioxide concentration led to work on the minimum concentration of carbon dioxide in illuminated leaves. In the field of stomatal physiology Dr. Heath's work is unrivalled. His recent demonstration of the role of chelation in the action of growth substances has opened a new line of approach. He took an active part in the work of the department both as lecturer and supervisor of postgraduate students.

The Atomic Energy Authority

Replying for the Prime Minister to a question in the House of Commons on January 24, Mr. R. A. Butler said he had no reason to suppose that the Prime Minister would dissent from the action proposed by the Atomic Energy Authority in a letter received from its chairman. In that letter, of January 21, Sir Edwin Plowden said that the Authority endorsed and will follow the principles of organization laid down in the report of the Fleck Committee appointed to inquire into the organization of certain parts of the Authority, and it also accepts in general the scheme of organization recommended for the Industrial Group and expects, in large measure and over a period of time, to implement the Committee's recommendations. It will endeavour to approach as nearly as possible the organization visualized by the Committee, having regard to the individuals who hold or could be found to fill the senior posts. The Authority is already embarking on a programme of recruitment from outside and of transfer within its own organization. On January 27, Mr. Maudling, replying to a parallel question, said that the Government and the Authority are still considering the implementation of the recommendations of the report of the Fleck Committee which investigated the health and safety arrangements of the Authority.

In reply to a further question about nuclear power station contracts, Mr. Maudling said that besides the four consortia formed in the earlier stages of preparation for the nuclear programme and trained by the Atomic Energy Authority in collaboration with the Central Electricity Authority, a fifth consortium has since been formed and trained at its own request. For technical reasons the design and construction of a nuclear power station can best be undertaken at present by such groups of companies. No exclusive rights are granted to the use of the Authority's information. The system will be kept under review, and more conventional methods may be possible later.

Exchange of Information on Thermonuclear Fusion

A JOINT United States–United Kingdom conference to exchange classified information on research in the field of controlled thermonuclear reactions was held during February 3–5 at the Naval Research Laboratory in Washington, D.C. The exchange of information is under provisions of the agreement for co-operation on civil uses of atomic energy between the two countries. This is the sixth such meeting since the co-operative programme between the two nations was inaugurated in October 1956.

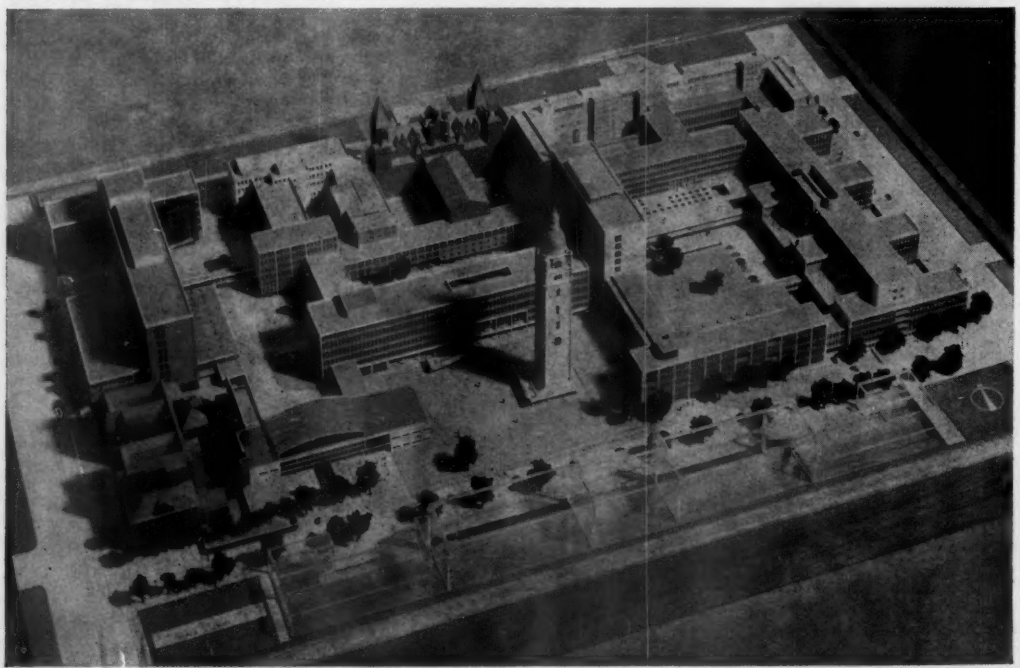
In the British delegation were: J. R. Bickerton and G. N. Harding, Atomic Energy Research Establishment, Harwell; R. F. Hemmings and B. S. Liley, Associated Electrical Industries, Aldermaston; H. Bodin, Atomic Weapons Research Establishment, Aldermaston. The United States were represented by teams of scientists led by A.E.C.-supported projects, including the Los Alamos Scientific Laboratory, Oak Ridge National Laboratory, Princeton University, the University of California Radiation Laboratory, New York University and the Naval Research Laboratory. Leaders of the groups attending were: Dr. James L. Tuck, Los Alamos Scientific Laboratory; Dr. E. D. Shipley, Oak Ridge National Laboratory; Dr. Lyman Spitzer, jun., Princeton University; Dr. Chester Van Atta, University of California Radiation Laboratory; Dr. H. Grad, New York University; Dr. W. R. Faust, Naval Research Laboratory.

Development of the Imperial College of Science and Technology

THE revised scheme for the development of the central site of the Imperial College of Science and Technology, London (see *Nature*, 177, 723; 1956; and 178, 123; 1956), in the rectangle bounded by Exhibition Road, Imperial Institute Road, Queen's Gate and Prince Consort Road, has now been placed before the London County Council for planning approval. The scheme retains the tower of the Imperial Institute Building, designed by Collcutt, as a central feature in the form of a free-standing campanile rising from the central court, in accordance with the recommendation of the Royal Fine Arts Commission. The Royal School of Mines building designed by Aston Webb, and the new building for the Departments of Chemical Engineering and Aeronautics designed by Norman and Dawbarn, have been incorporated into the scheme together with certain other buildings which do not form part of the College, notably the Royal School of Music on the northern side of the site. When completed, the new buildings will accommodate 3,000 students and will have a daily working population of more than 4,000 people. Work on the scheme, recently approved by the London County Council, for the development of Prince's Gardens, just off Exhibition Road, as halls of residence for the College is due to start this year. It will provide, in the first instance, residential accommodation for nearly 1,000 students. The plans embody a new conception of halls of residence, interpreting in contemporary terms the staircase system of the older universities.

Liberal Education in Technical Colleges

SOME useful suggestions about the place of liberal studies in technical education were made by Mr. A. A. Part, head of the Further Education Branch of the Ministry of Education, at King's College, Newcastle upon Tyne, on November 19. The education of a free man, he suggested, should be concerned with four



Model of the revised plan for the development of the Imperial College of Science and Technology: view from the south-west

essential features. First would be the development of self-respect. Second would come the stimulation of independent thought, and thirdly, closely allied to it, the fostering of the spirit of enterprise and imagination. Fourthly, the free man, in the circumstances of to-day, when all activities are becoming increasingly complex, must be able to work as a member of a team. In addition, many people would consider that to be truly free, a man needs the support of religion; one of the greatest reforms of the Education Act of 1944 was to secure for every schoolchild the opportunity for religious worship and religious instruction in school.

Institute of Information Scientists

At a meeting held at the Institution of Electrical Engineers on January 23, which some 125 persons (mostly graduate information officers) attended, Dr. G. Malcolm Dyson took the chair, and opening speeches were made by Mr. J. Farradane (Tate and Lyle, Ltd.) and Mr. C. W. Hanson (British Scientific Instrument Research Association). The proposal "That a professional body be, and is hereby, set up to promote and maintain high standards in scientific and technical information work and to establish qualifications for those engaged in the profession" was carried. The new professional body has been named the Institute of Information Scientists, and a temporary working committee has been appointed to devise a constitution for approval by a later meeting, when the permanent officers will be elected. Further information about the Institute can be obtained from Mr. J. Farradane, "Torran", Crofton Road, Orpington, Kent.

International Instrument Show

The International Instrument Show will be held in Caxton Hall, London, during the last week of March, opening on March 24. It is expected that more than sixty factories from ten countries will be participating, and their products will include a number of the latest technical achievements in the fields of printed circuits, ultra-violet television microscopy, electron paramagnetic-resonance spectroscopy and telemetry. This year greater emphasis has been placed on the attendance of factory engineers. Normal day opening hours will be from 10.30 a.m. to 6.30 p.m., but to facilitate the attendance of parties of students and apprentices, the show will remain open until 9 p.m. on March 26 and also until 12 noon on the following Saturday morning. Admission is free to *bona fide* applicants. Tickets are available from the sponsors, B. and K. Laboratories, Ltd., 57 Union Street, London, S.E.1.

Humphry Davy Lectures

The London Section of the Royal Institute of Chemistry is sponsoring a new series of lectures, to be called the Humphry Davy Lectures. The first, entitled "Purity and Purification of Chemical Substances", will be given by Prof. Arne Tiselius (University of Uppsala) at the Royal Institution, 21 Albemarle Street, London, W.1, on February 19 at 6.30 p.m. The chair will be taken by the president of the Royal Institute of Chemistry, Prof. W. Wardlaw.

Dr. W. J. Jongmans

Prof. John Walton, of the University of Glasgow, writes: The obituary notice by Alan Wesley which appeared in *Nature* on January 18 gave an interesting account of the work of Dr. Jongmans, but it should also be recorded that Jongmans was largely responsible for organizing the very successful and memorable series of congresses on Carboniferous stratigraphy held at Heerlen in 1927, 1935 and 1951. The idea of holding such a Congress was the result of conversations between Jongmans, Prof. Armand Renier and Prof. Walther Gothan. Jongmans, however, was responsible for practically all the organization and the editing of the voluminous *Comptes Rendus*. The congresses brought together those interested in Carboniferous geology from many parts of the world and the delegates received much hospitality from the City of Heerlen and the State Mines. In 1938 the University of Glasgow conferred on Jongmans the honorary degree of LL.D.

Dose-Rate Effects in Radiation-Induced Chemical Reactions

Referring to this article by Dr. H. C. Sutton and Prof. J. Rotblat (*Nature*, 180, 1332; 1957), Mr. J. P. Keene (Physics Research Department, Christie Hospital and Holt Radium Institute, Withington, Manchester 20) writes: "I have already published (*Rad. Res.*, 6, 424; 1957) an account of some measurements of G^{Fe} at 10^{10} rads/sec. The value obtained was 14·3, which is probably not significantly different from the value of 15·45 obtained by Schuler and Allen (*J. Chem. Phys.*, 24, 56; 1956) for dose-rates in the range 10^{8}–10^{9} rads/sec., and values in the region of 15 obtained by other workers at lower dose-rates. Thus we may conclude that most of the fall in the ratio G^{Fe}/G^{Ce}, due to electron irradiation in the range 10^{3}–10^{10} rads/sec. observed by Sutton and Rotblat was in fact due to an increase in G^{Ce}".

Announcements

Dr. G. E. R. Deacon, director of the National Institute of Oceanography since its foundation in 1949, has been elected a foreign member of the Swedish Royal Academy of Sciences.

Dr. Detlev W. Bronk, president of the U.S. National Academy of Sciences and also president of the Rockefeller Institute for Medical Research, has been awarded the Gold Medal of the International Benjamin Franklin Society.

A conference on "Fundamental Aspects of the Dehydration of Foodstuffs" will be held at the University of Aberdeen during March 25–27. Further information can be obtained from the Assistant Secretary, Society of Chemical Industry, 14 Belgrave Square, London, S.W.1.

The Institution of Electrical Engineers Faraday Lecture for 1957–58 will be given at the Central Hall, Westminster, at 6 p.m. on February 12. The subject will be "The Electrification of Railways". The Lecture was prepared by the late Mr. G. H. Fletcher and will be delivered by Mr. R. Ledger. Admission is free, by ticket obtainable from the Institution of Electrical Engineers, Savoy Place, London, W.C.2.

The title of the book by L. Saunders and R. Fleming noticed in *Nature* of January 18, p. 147, was given incorrectly: it should read "Mathematics and Statistics for Use in Pharmacy, Biology and Chemistry".

FLOW NOISE

DURING the latter part of the last century, the great scientists who studied acoustics attacked and solved most of the problems of acoustic excitation and propagation, the one notable exception being that of sound created by moving air. Since that time, partly because of its relative unimportance, but chiefly because of its intractability, the acoustician has only tackled the problem in a physical manner to explain the discrete frequencies which occur when air blows over wires (aeolian tones) or on to a sharp edge (edge tones) and has not at any time appealed to mathematics to obtain quantitative solutions. Since the Second World War, the greatly increased power used in civil airliners, made possible by the invention of the jet engine, has brought forth a noise situation in which the enormous power developed has more than outweighed the inefficiency of conversion of kinetic energy and has precipitated a major conflict between the flying and non-flying public. A detailed study of jet noise has thus become urgent, and this has brought into the field of acoustics those scientists who have, by tradition, been most knowledgeable about the details of the source, namely, the fluctuating flow in the jet as it mixes with the surrounding air. Since the acoustic output of a typical jet engine consists of only one-hundredth of 1 per cent of the total engine-power, this has involved a highly mathematical treatment. Hence, in Britain, the acoustician with his physical interpretation and the aerodynamicist with his mathematical approach have tended to keep apart, even while dealing with the same problem.

Thus it was that, at the joint meeting of the Acoustics Group of the Physical Society and the North-eastern Branch of the Institute of Physics held in Newcastle upon Tyne during November 15–16 under the joint chairmanship of Prof. E. G. Richardson and Dr. O. P. T. Kantorowicz, the major outcome was the cross-fertilization of ideas arising from these two groups. Indeed, as Prof. E. J. Richards (Southampton) pointed out in his introductory address, noise suppression has now become a study of turbulent mixing and has moved some way from pure acoustics. Thus, whereas the two-day conference was split into sessions on noise in air and in water, respectively, the natural gulf was, in fact, that between papers on turbulence and on discrete-frequency sound.

In the first category, considerable interest was aroused by three papers concerned not with noise and its propagation but with the aerodynamics of jet flow. In his talk, Dr. G. M. Lilley (Cranfield) examined the terms of Lighthill's equations and from them put forward valuable ideas regarding the essential parameters on which jet noise is based. He also examined in more detail than in the past the nature of the turbulence of the stream and its significance in contributing to the quadrupole sources of sound. Probably his most interesting proposal concerned a suggestion recently made by Townsend, that eddies emanating from a jet can be likened to long vortices stretching downstream with their axes along the line of the jet. He speculated that since these supply the major energy-carrying eddies, they are probably the greatest sources of sound; the obvious corollary to this is, of course, that noise-suppression techniques should be based on the deliberate destruction of these toroidal vortices, presumably by cross-flows and quick mixing systems.

In this category of papers on turbulence and its relationship to noise were also two papers from the United States: Dr. R. H. Kraichnan, of the University of New York, talked about the pressure and sound fields of turbulence with high Reynolds number, while Dr. E. Skudryzk, of Pennsylvania State University, presented some most interesting results regarding the noise produced by the boundary-layer turbulence of a rotating cylinder. Undoubtedly these three papers, involving the relationship between wide-spectrum noise and turbulence, indicate the direction of future flow-noise researches at least for the next five years.

The other papers presented in this two-day conference dealt almost exclusively with discrete-frequency sound phenomena. Such phenomena are, of course, much easier to picture physically and were received with considerable interest by the less mathematical members of the conference. Edge tones in water were described by Mr. M. Gross (Newcastle). Dr. J. H. Gerrard (Manchester) described his experiments on the singing of wires (aeolian tones); the hydrodynamic oscillator was described by Dr. G. G. Parfitt (Imperial College of Science and Technology, London) and the effect of nicking on organ flue pipes by Dr. D. M. A. Mercer (Southampton). It is impossible in the space available to describe these interesting contributions, nor the general survey by Mr. M. P. Tulin of the water-noise researches being carried out in the U.S. Naval Research Laboratories, in any detail. Dr. Parfitt's description of the work at Harvard in developing a hydrodynamic oscillator which provides a sound level of 165 decibels in air for the expenditure of only 14 watts in energy shows how noise and pressure resonances, normally a nuisance, can also be harnessed to provide a very efficient means of energy-transfer.

As would be expected in the study of sound by aerodynamicists, optical schlieren techniques have become the accepted method of studying discrete-frequency phenomena. One of these phenomena arises from the sound which is given out when an eddy passes through a shock wave. Since this sound

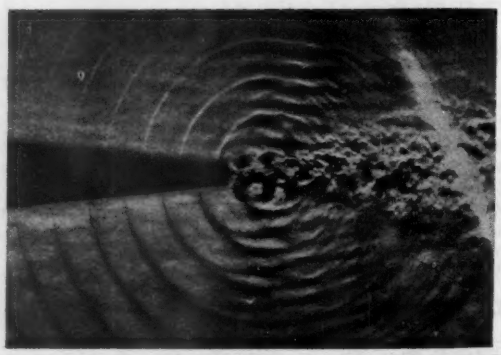


Fig. 1

wave proceeds upstream and produces a fluctuating flow, it is easy to see that for some frequency a self-sustaining system or resonance will occur. Two somewhat similar papers on this subject were presented at the conference, one by Mlle. Merle (Marseilles) and Mr. M. G. Davies (Liverpool). Both authors described their work on the 'back reaction' or resonance mechanism, Mlle. Merle showing a most interesting film of the cellular pattern in the jet. Mr. Davies gave more attention to the geometry of the system and a method of prediction of the frequency. The standard of photography was excellent in both cases; a typical photograph by Mr. Davies showing the sound waves emanating from the choked jet stream is shown in Fig. 1.

During the conference, members were conducted around the laboratories of the Pametrada Research Station and the cavitation tunnel at King's College, Newcastle. It is to be hoped that this meeting will be only one of a series drawing together workers in acoustics and aerodynamics, separated as they are now by the wide gulf of tradition.

ENZYME CHEMISTRY

AN international symposium on enzyme chemistry organized by the Science Council of Japan was held under the auspices of the International Union of Biochemistry in Tokyo and Kyoto during October 15–23. The president of the symposium was Dr. Seiji Kaya, president of the Science Council of Japan, and Dr. Keizo Kodama was chairman of the organizing committee. The meeting was reported to be the largest international gathering of scientists held in Japan since the War, 108 non-Japanese from twenty-one countries and 816 Japanese being present. The attendance from Western European countries was regrettably small, about half the overseas visitors being from North America, eight from the Soviet Union and numbers of from one to four from other countries. The thoughtfulness of the Japanese in presenting their work for the most part in English went a long way towards overcoming the language barrier, and their friendliness and the very great care with which our Japanese hosts had evidently planned every detail of the excellent organization made for an exceptionally smooth-running conference. Both at the University Club at Tokyo and within the University buildings at Kyoto, large and well-equipped reception and meeting rooms were provided. All participants received on arrival a massive volume containing the text of some 150 communications; the printed proceedings are to be published later and will include the discussions which followed the presentation of these papers.

Special lectures were given by Britton Chance on cytochromes in living cells, by H. Tamiya on the koji as a source of enzymes in Japan, by W. A. Engelhardt on enzymology and mechanochemistry, and by F. Lynen on the phosphate cycle and the Pasteur effect. H. Theorell and F. Lipmann, who were to have delivered conference lectures, were, unfortunately, unable to attend.

The proceedings were grouped under four main topics: mechanism of enzymic group transfer; enzyme systems of hydrogen, oxygen and electron transfer; formation of proteins and enzymes; enzymes and industry.

It would be impossible to mention here more than a few of the individual contributions, most of which maintained a high scientific level. The enzymic synthesis of polynucleotides was described in papers by A. Kornberg and S. Ochoa; lipoic acid and its functions in keto-acid metabolism by L. J. Reed and I. C. Gunsalus; pyridoxal-catalysed enzymic metabolism of amino-acids by E. E. Snell, A. E. Braunstein and K. Ichihara and their colleagues. Japanese work on cytochromes was reported by K. Okunuki, M. Ishimoto and S. Minakami, among others. A striking feature of the exhibition held in connexion with the symposium was the display of crystalline cytochromes by Japanese workers. In the same exhibition was a surprising range of crystalline proteolytic and other enzymes, of which some were commercially available, large samples being generously presented to visitors. Japanese work of industrial importance on the fermentative production of adrenaline and glutamic acid was presented at the scientific meetings.

Further papers concerned oxidative phosphorylations, to which subject outstanding papers were contributed by D. E. Green, B. Chance, A. L. Lehninger and E. C. Slater; while various other aspects of phosphoryl group transfer were described by A. H. Ennor, H. Tamiya and O. Hoffmann-Ostenhof. Biosynthesis of peptide bonds in relation to cell-structure was described by N. M. Sissakian and by T. Peters, and W. E. Knox gave a valuable account of adaptive enzyme formation in animals. The three members from the United Kingdom (F. Dickens, M. R. Pollock and C. Rimington) also presented papers.

At the closing session, Prof. A. Akabori, chairman of the Executive Committee, to whom must go much of the credit for a splendid job of organization, summed up the events of the symposium, and Prof. M. Florkin as president of the International Union of Biochemistry eloquently put the appreciation of the Union. The thanks of non-Japanese members were expressed by Prof. A. K. Balls who, on their behalf, presented to Prof. Kodama an illuminated scroll containing the signatures of the overseas members together with a brief message of appreciation in Japanese, offering their thanks and congratulations on a remarkably valuable and enjoyable symposium.

A full and varied programme of social events was provided, including a number of receptions, displays of Japanese dancing, the tea-ceremony and other picturesque traditional Japanese customs. Many overseas visitors were also able to enjoy the unique experience of life in a ryokan, or Japanese-style hotel, a charming and restful contrast with the bustle and noise of modern Tokyo.

Very attractive glimpses of the beautiful Japanese countryside were provided by excellent rail and bus excursions to the National Park at Nikko, to the ancient city of Nara and to the extremely interesting pearl-fisheries at Kashikojima.

After the symposium, three short further meetings were held. In Kyoto, Prof. Yas Kuno presided over a first-class two-day conference on water-soluble

vitamins, organized by the Vitamin B$_{12}$ Committee of Japan, which was attended by about fifty people. This is, of course, a field of intense Japanese activity, and some good Japanese papers were presented.

In Osaka, a one-day meeting on starch and amylases was held under the chairmanship of Dr. Shiro Akabori. At another informal but valuable discussion, on "The Metabolism of Cyclic and Sulphur Amino-acids", also held at Osaka on October 26, the contributors included A. Braunstein, [the late] C. Fromageot, W. E. Knox and K. Ichihara. All three of these meetings served to confirm the strong impression already formed by the visitors of the great energy and numerical strength of biochemists in Japan, and of their often outstanding technical ability which enabled them triumphantly to surmount the rather widespread difficulties due to old equipment and laboratories, many of which were not really adequate as judged by current European and American standards. In spite of these handicaps, to which must be added the low salaries of academic workers and the difficulty experienced by younger scientists in obtaining suitable posts at home and, still more, grants for travel abroad, the visitor could scarcely fail to be filled with admiration on observing the tenacity with which the Japanese workers pursued their researches, undaunted by these somewhat formidable difficulties. I am grateful to my two English colleagues at the symposium for their comments on this necessarily incomplete report. On the whole, this was a most rewarding symposium and a unique experience for most Western visitors.

F. Dickens

RESTORATION OF AGRICULTURAL AND OTHER SOILS

ONE of the problems in connexion with the restoration of soil that has suffered a major disturbance is the length of time which elapses before full fertility can be restored. Two papers dealing with aspects of this problem were read at a meeting of the Association of Applied Biologists in Leeds on September 19. The first of these was by R. Holliday and D. R. Hodgson on "Restoration of Land covered by Pulverized Fuel Ash". This paper dealt with investigations in progress at the University of Leeds under a research grant from the Central Electricity Authority, which is the main producer of fuel ash of this type.

The ash occurs in lagoons, into which it is washed after separation from the spent gases with which it is discharged. Utilization or disposal of this deposit after the lagoons are full is a major problem; its use for filling quarries involves costly transport, and wide-scale industrial use appears unlikely. Possible agricultural uses are therefore under investigation.

The agronomic problem posed is one of growing plants in material with the following properties: it consists mainly of spherical particles between 0·2 and 0·002 mm. in diameter, chiefly silica but with various metals incorporated; normal soil structure is lacking; it sets hard when dry but can be eroded by wind unless sealed with soil; the *p*H is high, varying between 8·5 and 10·5; it is deficient in plant nutrients and contains toxic ingredients; microbiologically it is completely sterile.

The problem of making it suitable for crop growth at a low cost is being investigated along these lines: reclamation through the use of a soil cover and fertilizer application; amending the ash as a growing medium by making up nutrient deficiencies, correcting toxicities and improving the structure; selection of crop plants tolerant to the adverse conditions.

Results are now available on soil cover trials using depths of up to 2 ft. of subsoil and 1 ft. of surface soil with four different surface concentrations of complete fertilizer. The crop-yield was affected with cover depths of less than 2 ft. of soil plus subsoil, but little beyond this level. The highest yields were obtained with a deep soil cover and a high fertilizer rate. Moderately good yields were obtained, however, from either a deep soil with moderate fertilizer rate or a shallow soil covering with a high fertilizer rate. Soil is often a scarce commodity and transport is costly, therefore the second alternative is more economical. All rates of fertilizer application gave poor results where no soil cover was used. The material was found to be deficient in nitrogen and phosphate, but potash is generally adequate.

Different crop plants have shown marked variation in the degree of root tolerance to the ash, but it is not clear whether this is a toxicity tolerance or a reaction to the physical structure of the growing medium.

Various methods to reduce alkalinity have been tried, in most cases with beneficial results. The question of cost, however, rules them out for large-scale use.

Dilution of the ash by the addition of bulky materials, including neutral and acid organic materials, shales and sewage sludge has given promising results in some cases.

Field trials on the capacity of various crops to grow in the ash have shown some plants of the orders Chenopodiaceae, Cruciferae and Leguminosae to be reasonably tolerant to the adverse growing conditions. Other crucifers and grasses have intermediate tolerance, while a wide range of economic plants, including cereals and pulse crops, are exceedingly sensitive. It has been shown that a period of weathering makes the growing conditions less unfavourable, and allows more of these plants to be grown with reasonable success on the ash.

The second paper, on "Some Biological Aspects of Opencast Coal Working", by H. W. Thompson, dealt with changes which occur in surface soil during opencast operations, resulting in lowered fertility and other undesirable qualities which persist for some years. Many factors are involved, some of which follow upon changes in the fauna of the soil.

Biological problems may arise in various ways. Many restored sites suffer from wetness, because permanent drains destroyed by opencast mining cannot be replaced until soil settling has ceased. Crops grown on undrained land frequently lack vigour and so are more liable to suffer pest injury, particularly by such pests as leather-jackets or slugs. Similarly, frit fly attack has often ruined spring oat-crops which, because of adverse soil conditions, have

been slow in establishment, with poor capacity for producing tillers.

Serious loss of crops arising in this way can often be avoided by care in cropping or, where appropriate, by the use of insecticides. The wetness problem may be solved in some cases by mole draining until such time as permanent drains can be replaced.

A different type of problem occasionally arises through the introduction of harmful organisms to an area not previously affected. There is an obvious risk, for example, of cyst-forming eelworms being spread when infested and non-infested soils are stored together and later distributed. Cysts of some of these eelworms can survive for twelve years or more in the soil. This aspect is therefore being studied, particularly in the case of the potato root eelworm.

This problem is not common at opencast sites, but has arisen in Lancashire where opencasting is in progress in an intensive arable area. Potatoes are an important crop and fields free from infestation are therefore particularly valuable. The spread of infestation is being studied at one such site. Samples of all fields to be worked were taken before scraping commenced. These showed some fields to be free from measurable infestation while others yielded high cyst counts. These fields will be re-sampled after restoration has been completed.

It was thought possible that cyst viability falls off more rapidly under spoil heap conditions than it would normally in arable land, especially where storage continues for several years in large heaps. At one site there was a definite indication that this does occur. Hatching tests on cysts collected from the top foot of soil showed moderate viability; samples from 8 ft. depth yielded cysts of very low viability, and those from 3 ft. depth showed intermediate larval emergence. The evidence so far from this, and from other sites where soil from heavily infested fields was stored, shows that the risk of spreading serious eelworm trouble as a result of opencasting is not very great, especially in view of the long rest from potato growing which is involved.

Another factor which is regarded as very important in its effect on soil structure and fertility is the great reduction in numbers of earthworms brought about by opencast working. Work on this aspect was therefore started in 1956. The object is to study the number of earthworms during the operation itself and to continue to do so after restoration until fertility is restored or the earthworm population has reached stability. Earthworm collection is carried out by hand digging because of the unusual conditions prevailing in spoil heaps.

A site near Wigan is being used, the soil being a sandy loam with an earthworm population estimated at 609,800 per acre before stripping commenced in May 1956. Counts on two spoil heaps and on an unworked area alongside have been made at roughly monthly intervals since. One spoil heap with a surface area of 1·2 acres is made up of surface soil from 11 acres, and the second, of 0·61 acres, from 4·8 acres.

Estimates made in August—four months after heaping—showed a low survival of earthworms, but by the following March numbers had increased to an estimated 77,000 in heap 1, and 43,000 in heap 2. Apart from a fall during the hot, dry period in June, numbers have remained at a relatively high level since, the highest estimates in both heaps and in the control area being obtained in March and October. A more complete picture of earthworm survival will be available after restoration has been carried out.

The opportunity was taken to study the earthworm position at a Barnsley site restored in 1952 after opencasting and cropped since then under twelve experimental rotations. Counts made in 1956, that is, four years after soil replacement, indicated that an earthworm population that could be regarded as normal for this type of soil had become re-established. The highest estimates were obtained from grazed leys with a population of 315,800 worms per acre, and nine of the twelve series of plots had counts exceeding 150,000 per acre. The information obtained from this site therefore suggests that re-establishment of earthworms after opencasting can be much more rapid than is generally believed to be the case.

H. W. Thompson

THE SCIENCE MASTERS' ASSOCIATION

ANNUAL MEETING

THE annual meeting of the Science Masters' Association, which was held during December 31, 1957–January 3, 1958, in the University of Leeds, was attended by more than five hundred science masters. Among those who came into residence in the University Halls were masters and students from Canada, South Africa, and Tasmania; the Royal Navy also sent its usual numbers from naval educational establishments.

The first evening began with the Association dinner, at which the toast of the Association was proposed by Prof. F. S. Dainton, chairman of the University School of Chemistry; this was responded to by the chairman of the Association, Mr. W. G. Rhodes, education and training officer of Arthur Lee and Sons, Ltd., steelmakers, of Sheffield. The members then assembled in the Parkinson Hall, where the presidential address was given by Sir Charles Morris, vice-chancellor of the University of Leeds.

Sir Charles was concerned with the fact that the external examinations undergone by schoolboys tend to be a test of the schoolmaster rather than of the pupils; a good master can cause indifferent pupils to pass; and he wondered whether science is not being taught too well. He thought also that too many facts are being taught, and that it is more essential to teach fundamental principles: as a philosopher, he thought that over-reliance on the memory is a very grave fault. This over-reliance is due largely to the great mass of facts which modern pupils are expected to assimilate: it is high time that dons and schoolmasters came together to reduce the content of the syllabuses. Sir Charles then dealt with the problem of what science should be taught to those who will not become professional scientists. Whereas the training in arts needed by the scientist is comparatively easily met by giving him a broad education in the use of language and the appreciation

of literature, it is not so easy to determine the solution of the converse problem. It is not sufficient to let the non-scientist do badly some diluted version of the scientist's course; what is needed, it seemed to Sir Charles, is the philosophical approach, whereby the great steps in the history of science, the modern applications of science, and its future possibilities are all freely discussed. He thought that science teachers tend to like to teach what they know with certainty, and that they are "deeply unwilling to go outside the field of precise knowledge". It is most important that the misunderstandings between scientists and humanists should be recognized as a deep and serious problem of education; it is something that we had even begun to think about it.

The mornings of the three succeeding days were occupied by lectures. These were by Prof. J. G. Wilson on cosmic ray astronomy, by Prof. L. N. Pyrah on scientific methods in urological research, Prof. F. W. Spiers on "Radiation and Man", Prof. J. T. Whetton on science applied to mining problems, Prof. E. A. Spaul on hormones and behaviour, Prof. E. G. Cox on magnetism and chemistry, Dr. F. E. Hoare on low temperatures as a research tool, Dr. Dorothy H. Rayner on problems of palæontology and evolution, Prof. R. D. Preston on physics applied to botanical problems, and Prof. J. B. Speakman on the chemical revolution in textile technology. Guest lecturers from outside the University were Mr. A. B. Thompson, of the Imperial Chemical Industries Terylene Council, who dealt with the physico-chemical principles underlying fibre-making processes, and Prof. L. Rosenfeld, of the University of Manchester, who spoke on Hellenistic science; the growth of interest in the history of science was shown by the size and membership of his audience.

The second evening-lecture, held under the auspices of the Science and Civilization Lecture Fund, was given in the Parkinson Hall by Mr. Ritchie Calder, who spoke on "The Presentation of Science to the Public". Mr. Calder dealt first with his own experiences in Israel when he was engaged in making plain to the Israeli public that a costly research institution was an asset to the State, even at a time when the State was fighting for its existence, and when the value of the scientific work undertaken might not have been immediately obvious. He found that the research scientists themselves are all too apt to think that any indulgence in publicity would lead to a lowering of their scientific standards. He went on to give other instances, not in Israel, where famous scientists had to be persuaded that it was very well worth while to give accounts of their work which could be understood by the ordinary newspaper reader.

The afternoons were occupied by visits to university departments and to factories and other works near Leeds; there were twenty-four of these, a record number. On the third evening the University held a reception for the Association; at this some three hundred members and their ladies were welcomed by the Chancellor, H.R.H. The Princess Royal.

An exhibition by publishers, manufacturers, and members occupied a large space in the chemical and physical laboratories; there were also special exhibitions by the Atomic Energy Research Establishment, the Aluminium Development Association, and the Copper Development Association.

A programme of scientific films, rather larger than usual, included one entitled "Galileo's Law of Falling Bodies", made by Encyclopædia Britannica Films, Ltd., in the United States in conjunction with the Ford Foundation; this forms part of a filmed teaching course of physics, which will be obtainable in Britain from the Gaumont British Film Library.

The final morning session ended with a demonstration and a discussion, which, owing to limitations of time, had to be held simultaneously.

The very striking demonstration, entitled "Biology for Non-Biologists", was given by Mr. C. H. Bailey, senior lecturer in science at Didsbury Training College; his experiments were carried out with simple apparatus and are intended for those teaching in secondary modern schools, where only a minimum of equipment may be available.

The discussion was held by the Association's Science and Religion Group, and was initiated by the Rev. G. W. Burningham, who acts as chaplain to the Anglican students at the University of Leeds. He emphasized how much greater was the interest of students in religion than it was twenty years ago, and that, if anything, the science students were more keen than the others; there is now more realization of the interdependence of scientific discoveries and religious beliefs. The ensuing discussion ranged over such subjects as miracles, the nature of the soul, and definitions of Heaven and of eternal life.

At the business meeting, Sir Lawrence Bragg was elected president for the ensuing year, and Mr. Charles Holt, of Harrow Weald County School, was elected chairman. The treasurer, Mr. S. W. Read, reported a very favourable balance on the year's working; and it was announced that the next meeting would take place in University College, London, during December 30, 1958–January 2, 1959.

THE METEOROLOGICAL OFFICE

ANNUAL REPORT, 1956–57

SOME years ago a prominent authority was heard to suggest that the annual report of the Meteorological Office might well provide a splendid subject for discussion at a meeting of the Royal Meteorological Society; for every year it gives food for thought. Englishmen regard the weather as their common property; and informed scientists cannot but admire the determination with which the public service endeavours to remain free from perturbation, whatever difficulties attend their efforts to cope with the vagaries of atmospheric behaviour. The Meteorological Office, in the nature of things, is always on guard duty. Day by day its actions, like those of the sentries at Buckingham Palace, are subject to every variety of comment from the bystanders. Fortunately there are always some with enough knowledge to admire a difficult task well done; and to recognize that the demands of a modern community continue to add to the difficulties. Forecasting the winds for a jet bomber at 50,000 ft. is a

requirement far beyond the conceptions of those who, sixty years ago, were willing to estimate in what parts of the country the term 'muggy' might apply, on the assumption that the oncoming depression would resemble Abercromby's classical model.

The annual report of the Meteorological Office for the year ended March 31, 1957*, shows admirably, with a subdued but unmistakable note of confidence, that the public service continues to make that sober progress which is to be desired on the part of those who study turbulent motion. As Sir Graham Sutton says, the meteorological problem is probably the most difficult in classical physics, and progress can only be made with the utmost diligence. Forecasting naturally attracts most attention, and efficient accomplishment in dealing with stupendous quantities of incoming factual material will be greatly aided as newly installed electronic computers come more into use. The development and direction of an active cadre of young research workers will ensure that in an accelerating world we shall not only keep up with our neighbours, but may here and there set the pace.

* Annual Report on the Meteorological Office for the year April 1, 1956, to March 31, 1957. Pp. 51+4 plates. (M.O. 633.) (London: H.M. Stationery Office, 1957.) 3s. net.

Readers of the report cannot fail to be impressed by the variety of services provided and by the diversity of research; for example, "experiments with the release of silver iodide from ground generators on Salisbury Plain continue with no result yet detectable". Elsewhere is mentioned the work on forecasting the outlook for periods up to a month ahead; "the outlook on this stubborn problem is rather brighter than hitherto".

The British business man is too often insufficiently aware of these public services; and the potential rewards of successful forecasting offer abundant temptation to incautious consultants. There are those who, to all appearance, gamble on sunspots; those who believe it has been given to them to discover periodicities of various sorts; those who think in terms of singularities, and perhaps some on the outskirts who appear to be best described as 'hunch-drunk', to use the delightful phrase coined by an eminent professional meteorologist. But how many of our would-be 'independent consultants' have the temerity to disregard the world synoptic charts, so efficiently assembled by the Meteorological Office working with other nations in an unostentatious but exhilarating daily display of international collaboration?

GORDON MANLEY

RÉAUMUR

BESIDES his thermometer, René-Antoine Ferchault, lord of the manor of Réaumur in the Vendée, and of Des Angles and La Bermondière, made many contributions to scientific knowledge. In 1708, at the age of twenty-five, he presented a memoir to the Academy of Sciences, and in 1709 two others concerned with general solutions of certain geometrical problems. Having justified his election as *élève* in geometry, he then turned his attention to natural history, and in some highly original observations demonstrated that the shells of molluscs grow by successive external additions of new parts rather than by the alternative process of intussusception. On the death of the geometer Louis Carré in 1711, he was promoted to *pensionnaire mécanicien* and put in charge of the work on trades, arts and crafts; he compiled volumes on the manufacture of anchors, of pins and of cast iron.

In the next few years Réaumur discovered an insect living as an internal parasite on the snail; he studied the movements of shell-fish, sea-anemones and starfish, and the great variety of organs of locomotion in shell-fish and their means of attachment to different solid bodies; he found a hitherto unknown fish that gave a purple colouring matter like the famous Tyrian purple of the ancient world; he demonstrated the correctness of the popular belief that crayfish, crabs and lobsters, having lost a claw, could grow another in its place; he studied the torpedo fish, or electric ray, known for its property of benumbing the hand or limb that touched it, and, while the usual explanation was that this was due to the emission of 'torporific corpuscles', he argued that it was produced by the speed and rapidity of the blow that the ray could inflict by means of its admirably constructed muscular system; he examined the native turquoises of Languedoc, showed that they were in no way inferior to those imported from Persia, supposed at that time to be the only source of these gems, and demonstrated by experiments that they were petrified fossil-bones coloured by a metallic ingredient dissolved in them by firing; he studied pearls and their production and falsification, considering the former as due to disease and showing that the colouring matter of false pearls was obtained from the scales of a fish known as the ablette (in English, the bleak); he examined the great bank of *falun*, or fossil shell-marl, in Touraine; he was interested in the nature and formation of flint, in the plant called *Nostoc* or star-jelly, in the luminescence of certain shell-fish, in the ease of magnetization of iron and steel by percussion, and in the recovery of gold from the sands of various French rivers; he had also already begun to reveal his active interest in the practical arts and their relation to the sciences, reporting to the Academy descriptions of the preparation of slate, and of the gilding of leather, of mining for iron-ore, of the manufacture of mirrors, of the drawing of gold-wire and the properties of paper and other bodies in preventing the passage of air and water.

Later, Réaumur published a paper describing the procedure for the conversion of wrought iron into steel: this is a landmark in the history of the iron and steel industry and effectively marked the introduction of the manufacture of steel into France.

Réaumur also rendered a similar service to the tinplate industry; he showed that vinegar was the cheapest and most effective solvent for the removal of the scale from the surface of the iron before tinning.

All this, as well as Réaumur's outstanding achievements with thermometers and in entomology, is described in a fascinating article by Prof. D. McKie published in *Science Progress* (45, No. 180; 1957). Besides this study of Réaumur, the journal contains an article by Prof. W. B. R. King on water supply and geology and one by Dr. G. H. A. Cole on the Earth and its magnetic field.

ORIGIN OF COMETS

"THE Distribution of the Directions of Perihelia of Long-Period Comets" is the title of a paper by J. G. Tyror in *Mon. Not. Roy. Astro. Soc.* (**117**, 4; 1957), in which he deals specially with Lyttleton's theory of the origin of comets (see *ibid.*, **103**, 465; 1948; and "The Comets and their Origin", Cambridge, 1953). This theory envisaged a process of accretion of dust particles by the Sun as it passed through interstellar dust clouds, and while it explained some of the observed features of comets and of their orbits, like many other theories it presented certain difficulties. If the theory is accepted, then comets must originally have come from preferred positions in the sky, and although the diffusion of the perihelion points of such a family would probably occur, it is considered that some traces of perihelion groupings, at least for families born during recent cloud passages, could be detected.

Mr. Tyror examines the distribution of perihelion points of long-period comets with the object of determining whether there is any evidence of such grouping. Previous investigations by W. H. Pickering and others suggested that there was some evidence of the grouping of aphelion points, but it has been considered advisable to revise these results in the light of additional information which has appeared since the completion of their work. The present survey does not include short-period comets and periodic comets because planetary perturbations would have eradicated reliable evidence of any original grouping; while it is true that all orbits would have been affected to some extent, the investigation has been carried out only for comets suffering slight perturbations. The whole subject is beset with difficulties because it is inevitable that available data must suffer to some extent from observational selection. A detailed description of the contents of the paper is quite impossible in a short note, and it will suffice to deal with the data utilized and the conclusions that have been reached.

Investigations of the positions of 448 comets with periods greater than 200 years were made, and of these 67 had calculated eccentricities greater than 1, 263 equal to 1, and 118 less than 1. There is evidence that the perihelion points of long-period comets tend to be distributed non-randomly, and it is pointed out that this tendency may be due to observational selection or to a real grouping phenomenon, or even to a combination of both. The first of these has undoubtedly played a prominent part in the discovery of comets, and other vitiating factors in the analysis are mentioned. The conclusions are summarized at the end of the paper and are briefly as follows.

The departure from randomness in the observed distribution of perihelion points of the comets considered is strongly suggested, and there is evidence indicating a preference for the perihelion points to lie near a particular great plane. In addition, the centre of the observed distribution appears to be significantly displaced towards a point on the celestial sphere. The identification of the plane with the plane of the Galaxy and of the point as that lying in a direction opposed to that of the Sun's motion cannot be considered unreasonable. These suggestions may be interpreted in the light of Lyttleton's accretion hypothesis, according to which the perihelion positions of comets should show a tendency to lie near the apex of the Sun's motion relative to the local interstellar cloud, and further, the distribution of perihelion points would have a preference for lying near the galactic plane.

QUALITY IN MEAT

NOW that the supply of meat is approaching the level of demand, quality is again assuming economic importance. Definitions and tests of quality have been examined by D. M. Joubert of the Department of Animal Husbandry, University of Pretoria, who has also examined the economic consequences which follow from changed attitudes towards quality (*Colonial Plant and Animal Products*, **6**, No. 2; 1956).

Only a few decades ago quality in beef meant well-marbled meat covered by a thick layer of fat. Not only did the average consumer during those times demand more fat owing to the bodily needs imposed by hard manual labour, but also excess fat was used for other purposes, chiefly cooking. At present, cooking fats can be obtained from cheaper sources, the consumers perform less manual labour and are better protected in their homes against cold conditions, and consequently demand the minimum of fat with their beef. Since butchers are concentrating on carcasses of young animals, it is not unlikely that the optimum carcass weight will become even lower in future.

When considering quality in the case of mutton and lamb, the size factor is of primary importance. With the size of the average family decreasing all over the world and with cooking facilities less spacious than they used to be, the general demand is for small joints. Preference is given to a whole joint of small size over half a large one since the latter, because of its cut surface, does not retain its succulence so well and leads to greater wastage through exudation. As in beef, a minimum of fat is preferred. With the rapid development of the dairy industry in recent years and with progress in the manufacture of margarine, the excess fat is no longer widely used as a substitute for butter and goes to waste.

Definition of quality in mutton is particularly complicated because of the differences in the taste of individuals in respect of flavour. Mutton produced from early-maturing sheep (as, for example, Southdown, Suffolk) raised and fattened in stalls may appear extremely attractive and, therefore, be labelled as of high quality. On the other hand, mutton produced from late-maturing sheep breeds (such as Scotch Blackface, and even possibly Merino) off extensive pastures may, by comparison, appear unattractive and may be labelled as being of low quality.

The quality of pork is determined primarily by the amount and nature of the subcutaneous back fat. The amount of fat preferred with pork by consumers in Britain at present is considerably less than a few decades ago, while in South Africa, for example, a large amount of fat has never been tolerated except

where it was required for purposes other than eating. It was not realized that lean pork can be produced provided the right type of animal is used and that correct feeding is practised, and the consumption of pork in South Africa remained extremely low until quite recently. On the contrary, in certain Continental countries the general taste demands rather more fat with pork, and a certain amount of excess fat is required by butchers for the production of a great many varieties of sausage and salami. Consequently the type of pig raised in the central European States (such as Germany and Hungary) differs markedly in its conformation from that which is considered as the ideal type in Great Britain and South Africa.

The final factors determining the quality of a piece of meat, and one which often nullifies all factors considered prior to its cooking, centre around the purely human element, namely, the ability of the cook—either to spoil the good qualities of meat through inefficiency, or to improve to such an extent the quality of a poor sample of meat through skill and knowledge of cooking, *per se*, that its pre-cooked quality remains unquestioned.

ASSAY OF VITAMIN A OILS

By Prof. E. BRUNIUS

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IN 1954 a Vitamin Assay Commission was set up by the Food Division of the Applied Chemistry Section of the International Union of Pure and Applied Chemistry.

The official members of the Vitamin Assay Commission are: Prof. E. Brunius (Sweden, chairman), Dr. W. F. J. Cuthbertson (Great Britain), Dr. M. Kofler (Switzerland), Dr. B. L. Oser (United States) and Prof. H. Simonnet (France). The Commission has held four Conferences: in London 1954, Zurich 1955, Amsterdam 1956, and Paris 1957. In all the Conferences, besides members of the commission, the following participated: Dr. D. W. Kent-Jones (Great Britain, honorary secretary of the Food Division), Prof. R. Nicolaysen (Norway, member of the Food Division), Mr. E. Hayes (Great Britain), and Mr. R. J. Taylor (Great Britain). In addition the following were present at the Amsterdam conference: Dr. J. Straub (The Netherlands, member of the Food Division), and Dr. C. Engel (The Netherlands); and at the Paris Conference: Dr. K. Dürrenmatt (Switzerland, chairman of the Food Division), Prof. J. Brüggemann (Germany), Dr. C. Engel (The Netherlands), Dr. A. François (France), and Dr. J. Tiews (Germany).

One of the Commission's tasks was to develop a standardized method for the assay of vitamin A oils which would be acceptable in international trade.

Current vitamin A assay methods are based on the recommendations made by the Subcommittee on Fat-Soluble Vitamins of the World Health Organization. In its report[1] the International Unit of vitamin A potency is defined in terms of the activity of crystalline all-*trans* vitamin A acetate, and the conditions for the spectrophotometric assay of the vitamin are broadly set down. The elaboration of methods in different countries, however, has led to divergences in the assessment of vitamin A potency, and this has given rise to difficulties in the international trade in vitamin A oils. It was therefore considered an urgent task for the Commission to attempt to develop a standardized method of assay.

Vitamin A oils vary widely in quality and potency. At the outset the Commission decided, on the evidence available, to consider them in two categories according as their spectrophotometric characteristics approached those of pure vitamin A or not. For oils in the first category (comprising most fish-liver oils and vitamin A concentrates) a correction procedure based on the Morton–Stubbs principle[2] was considered sufficient. For oils in the second category (comprising whale-liver oils and other oils having a high proportion of irrelevantly light-absorbing materials) a chromatographic step should precede the determination of extinction and the application of correction. All measurements should be made on the unsaponifiable matter of the oils. It was suggested that this should be isolated by the method described in the United States Pharmacopeia.

The Commission organized three collaborative experiments, in which laboratories in Europe, Japan and the United States co-operated. Thirty-seven laboratories participated in the first experiment, thirty-four in the second and nine in the third.

The main purposes of the experiments were: (1) to ascertain the loss of vitamin A in isolating unsaponifiable matter of oils according to the method of the United States Pharmacopeia; (2) to investigate whether *iso*propanol and absolute ethanol could be used as alternative solvents in measuring extinction; (3) to determine the constants for a correction formula based on the Morton–Stubbs principle[2]; (4) to ascertain the loss of vitamin A in a chromatographic method suggested for samples in the second category; (5) to investigate the selectivity of this method; (6) to compare two techniques for following the movement of vitamin A through the chromatographic column; and (7) to estimate the experimental errors in the vitamin A potencies of oils in both categories as assayed by the procedure suggested.

A vitamin A reference standard containing all-*trans* vitamin A acetate in vegetable oil (nominal potency 100,000 I.U. per gm.) and samples of oils belonging in the second category were used as test materials.

In the chromatography, the unsaponifiable matter of the sample was passed through a column of weakened alumina and the vitamin was eluted with mixtures of light petroleum and ether. The movement of the vitamin was followed by testing aliquots of the eluate with antimony trichloride solution and, in the third experiment, alternatively by observing the column in weak ultra-violet light. Two different samples of alumina were used by most participants.

A statistical screening procedure, based on Chauvenet's criterion[3], was applied to the results.

The main outcome of the experiments can be summarized as follows. The average loss of vitamin A in isolating the unsaponifiable matter of the vitamin A reference standard was found to be 1·2 per cent in

the first experiment and 1·2–1·5 per cent in the second. *Iso*propanol and absolute ethanol could not be recommended as alternative solvents, because the respective E_{310}/E_{325} and E_{334}/E_{325} ratios differed significantly in the two solvents, which gave rise to systematic errors when correction was applied. The correction formula derived for *iso*propanol from the results of the first and second experiments was:

$$E_{325}\ \text{corr.} = 6{\cdot}815\,E_{325} - 2{\cdot}555\,E_{310} - 4{\cdot}260\,E_{334}$$

The average loss of vitamin A in chromatographing the unsaponifiable matter of the vitamin A reference standard amounted to 2·4 per cent irrespective of the kind of alumina used. The average total loss of vitamin A, that is, the losses in isolating and chromatographing the unsaponifiable matter, was 3·5–4 per cent. After chromatography the samples of oils belonging in the second category readily met the specifications laid down for oils in the first category. The two procedures for following the movement of vitamin A through the column were equally efficient and could thus be recommended as alternative techniques. The inter-laboratory error (s) at $P = 0{\cdot}95$ for a duplicate assay of vitamin A potency was estimated at 6–8 per cent for an oil in the first category and at 10–14 per cent for an oil in the second category. The error of the mean of assays performed at n different laboratories—each of them making a duplicate assay—will accordingly be $s/\sqrt{n}$.

For vitamin A oils showing negligible amounts of irrelevant light absorption, the corrected E value should theoretically be identical with the uncorrected E value but, because of the experimental error due to the correction procedure, positive corrections must sometimes be encountered. About half the assays on the vitamin A reference standard in the first and second experiments showed positive corrections. In the first experiment 14 out of 15 positive corrections did not exceed 2·5 per cent and in the second 29 out of 30 positive corrections did not exceed 3·5 per cent.

The Commission now makes the following recommendations for the assay of vitamin A oils:

1. All oils shall be saponified, and the assay made on the unsaponifiable matter.

2. The unsaponifiable matter shall be isolated by the method of the United States Pharmacopeia XV [4].

3. All spectrophotometric measurements shall be made in *iso*propanol as solvent.

4. Oils shall be considered in two categories:

Category 1: those oils the unsaponifiable matter of which has an absorption maximum lying within the region 323–327 mμ and an extinction ratio E_{300}/E_{325} not exceeding 0·73.

Category 2: those oils the unsaponifiable matter of which has an absorption maximum lying outside the region 323–327 mμ or an extinction ratio E_{300}/E_{325} exceeding 0·73.

5. For oils in category 1, the E_{310}, E_{325} and E_{334} values of the unsaponifiable matter shall be measured and a correction applied according to the formula:

$$E_{325}\ \text{corr.} = 6{\cdot}815\,E_{325} - 2{\cdot}555\,E_{310} - 4{\cdot}260\,E_{334}$$

6. For oils in category 2, the unsaponifiable matter shall be chromatographed according to a prescribed procedure. Provided the chromatographed unsaponifiable matter satisfies the criteria for category 1 oils, its E_{310}, E_{325} and E_{334} values shall be measured and a correction applied as directed under 5.

7. Corrected E_{325} value shall be used for calculating vitamin A potency, except in the cases mentioned under 8.

8. A corrected E_{325} value which lies within ± 3·0 per cent of the uncorrected value shall be regarded as equal to the uncorrected value for the purpose of evaluation, and the uncorrected value shall be used for calculating vitamin A potency. A corrected E_{325} value which is greater than 1·030 times the uncorrected value shall be re-determined and, if confirmed, treated as a special case between buyer and seller.

9. The E_{325} value is converted into $E^{1\%}_{1\text{cm.}}$ 325 mμ. The vitamin A potency of the sample, expressed in I.U. per gm., is obtained by multiplying the $E^{1\%}_{1\text{cm.}}$ 325 mμ value by 1830 [5], that is, the factor of the reference quoted, taken to three significant figures.

Detailed reports of the Commission's collaborative work and of the recommended procedure for assay will be published elsewhere.

[1] Report of the Subcommittee on Fat-Soluble Vitamins, WHO/BS/65 Rev. 1 (1949); World Health Org. Tech. Rep. Ser., No. 3 (1950).
[2] Morton, R. A., and Stubbs, A. L., *Analyst*, 71, 348 (1946).
[3] Chauvenet, "Theoretical and Practical Astronomy", 2. Quoted from Brunt, D., "The Combination of Observations", 130 (University Press, Cambridge, 1923).
[4] "The Pharmacopeia of the United States of America", Fifteenth revision, 941 (Mack Pub. Co., Easton, Pa., 1955).
[5] Boldingh, J., *et al.*, *Nature*, 168, 598 (1951).

CORRECTION FOR THE GAMMA-RAY SENSITIVITY OF THE BETA-COUNTER IN THE STANDARDIZATION OF COBALT-60 BY THE BETA–GAMMA COINCIDENCE METHOD

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THE standardization of radioactive isotopes by the beta–gamma coincidence method involves correcting the observations to obtain the true disintegration rate[1,2]. Some of these corrections, such as the natural backgrounds in the beta- and gamma-counters, can be determined without difficulty. The correction for the γ-ray sensitivity of the beta-counter, however, is usually made by screening the counter from the source by placing between them an absorber of such a thickness that it absorbs all the β-radiation. The objection to this method is that the absorber usually alters the γ-ray sensitivity of the beta-counter.

In an attempt to solve this problem for cobalt-60 we have incorporated a pulse-amplitude analyser following the β-ray detector of the beta–gamma

coincidence-counting equipment. This equipment[3] consists essentially of two scintillation counters, one containing a thin anthracene crystal as the beta-counter and the other a thick crystal of activated sodium iodide as the gamma-counter.

Pulses lying within a voltage-range V to $(V + \Delta V)$ are passed by the analyser to the coincidence unit, and finally counted in the β-scalers. As V is varied, the β-counting rate will alter in a manner similar to the energy distribution of the original β-particles.

The beta-counter will also detect secondary electrons caused by the interaction of the cobalt-60 γ-rays with the crystal and surrounding material. It is the detection of these secondary electrons which gives rise to the 'γ-ray sensitivity of the beta-counter'. For cobalt-60 the energies of these electrons range from zero to 1·33 MeV., which is the energy of the higher of the two γ-rays. The β-particles range in energy from zero to 0·3 MeV. An absorber thick enough to absorb electrons up to 0·3 MeV. will cut off the β-radiation, but the residual counting rate with the absorber will not necessarily be the same as the γ-counting rate without the absorber.

We have measured the pulse-output distribution from the beta-counter, both with and without an absorbing screen in front of the anthracene crystal. Fig. 1 shows the result obtained. Without an absorber, a curve resembling the usual β-spectrum is obtained; with an absorber, the distribution appears flat. Measurements above the end-point energy of the β-spectrum shows that the γ-efficiency is increased by the interposition of an aluminium absorber 0·5 mm. thick between the source and the counter. This would also probably be the case when the analyser channel is set to record β-particles. That is, the standardization of cobalt-60 with this equipment, using a conventional absorber, will be erroneous because the γ-ray sensitivity of the counter is increased by the use of an absorber.

It is reasonable to assume that because the pulse distribution appears flat with an absorber, it will also be flat without an absorber (although not the same in magnitude). If this were so, then a variation of the analyser channel position would vary the β-efficiency of the beta-counter, but its γ-efficiency would remain constant (channel width ΔV and amplifier gain constant). This can be shown to be one of the conditions for the β-counting rate to be a linear function of the coincidence-counting rate.

For, considering the decay scheme shown in Fig. 2, if $(N_\beta + N_{\gamma \ln \beta})$ is the β-counting rate for any particular setting of the analyser channel, e_β the efficiency of the beta-counter for the β-rays, and $(e_1 + e_2)$ the efficiency of the beta-counter for the two γ-rays from cobalt-60, then

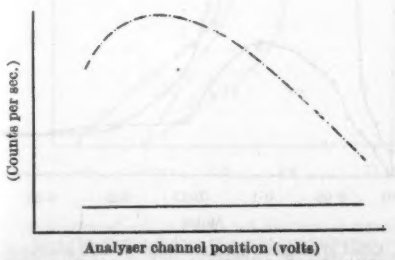

Fig. 1. Output of the anthracene β-detector with (full line) and without (pecked line) an absorbing screen

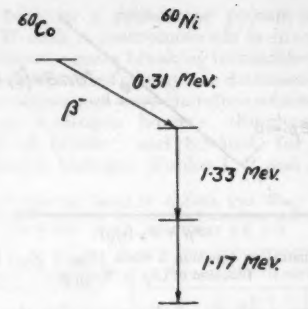

Fig. 2. Cobalt-60 decay scheme used for equation 5 (neglecting the weak β-branch)

$$(N_\beta + N_{\gamma \ln \beta}) = N_0' e_\beta + N_0 (e_1 + e_2) \qquad (1)$$

where N_0 is the true disintegration rate.

Similarly,

$$N_\gamma = N_0 (E_1 + E_2) \qquad (2)$$

where N_γ is the counting rate from the gamma-counter, the efficiency of which is $(E_1 + E_2)$ for the two γ-rays from cobalt-60. In the experiment, $(E_1 + E_2)$ is kept as constant as possible, that is to say:

$$N_\gamma = N_0 (E_1 + E_2) = \text{const.} \qquad (3)$$

If $(N_{\beta\gamma} + N_{\gamma\gamma})$ is the counting rate from the coincidence circuit,

$$(N_{\beta\gamma} + N_{\gamma\gamma}) = N_0 e_\beta (E_1 + E_2) + N_0 (E_1 e_2 + E_2 e_1) \qquad (4)$$

A re-arrangement of equations (1), (2) and (3) gives

$$(N_{\beta\gamma} + N_{\gamma\gamma}) = (N_\beta + N_{\gamma \ln \beta}) (E_1 + E_2) - N_0' (E_1 e_1 + E_2 e_2) \qquad (5)$$

Equation (5) expresses the relation between the β-counting rate $(N_\beta + N_{\gamma \ln \beta})$ and the coincidence counting rate $(N_{\beta\gamma} + N_{\gamma\gamma})$. It is assumed that the usual corrections for backgrounds, accidental coincidences and dead-time losses have already been made to the observations.

Equation (5), expressed in the form $y = mx + C$, will be linear when the terms corresponding to m and C are constant. It is arranged experimentally that $m = (E_1 + E_2)$ is constant; but in the case of $C = N_0 (E_1 e_1 + E_2 e_2)$, N_0 is a constant, allowing for radioactive decay during the measurement, therefore the required condition is that $(E_1 e_1 + E_2 e_2)$ must be constant. If $(E_1 e_1 + E_2 e_2) = \text{const.}$, all terms are finite and E_1 and E_2 are constant, then either $\Delta e_1 / \Delta e_2 = -E_2/E_1$ or $(e_1 + e_2) = \text{const.}$ If, therefore, the γ-spectrum of the beta-counter is indeed flat, then one of the conditions for $(N_\beta + N_{\gamma \ln \beta})$ to be a linear function of $(N_{\beta\gamma} + N_{\gamma\gamma})$ is realized. Furthermore, the slope $(E_1 + E_2)$, in conjunction with the γ-counting rate, will give N_0 without any necessity for using an absorbing screen. Fig. 3 shows a hypothetical plot of this linear function. The straight-line plot extends from $e_\beta = 0$ to an upper limit determined by the apparatus.

Measurements on cobalt-60 so far have shown that this line has a slight curvature, the slope varying by about 3 per cent over the range of measurement. A

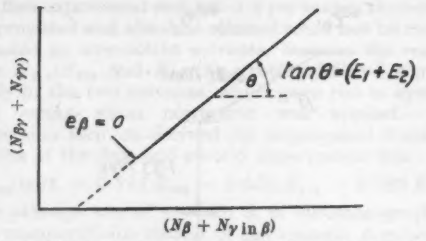

Fig. 3. Illustrating equation 5 when $(N_{\beta\gamma} + N_{\gamma\gamma})$ is a linear function of $(N_\beta + N_\gamma \ln \beta)$

least-squares analysis of the points on the line gives a standard deviation of ± 0·5 per cent for the slope, but in view of the curvature, the disintegration rate cannot as yet be determined to better than ± 1·5 per cent. The curvature appears to be influenced by several experimental variables which are at present being investigated.

Equation (5) can be investigated by other methods for varying e_β. For example, it is possible to vary e_β by placing absorbers of various thicknesses in front of the beta-counter, and it can now be seen that a measurement with an absorber which cuts off all the β-radiation represents only one point on this line; a measurement without an absorber constitutes another point, and the usual method of evaluating N_o from these two measurements is really a determination of the slope of this line. The apparent disintegration rate is less or greater than the true one according to whether the absorber increases or decreases the γ-efficiency of the beta-counter. This can be shown in a similar way to the argument involving equations (1) to (5), except that now the γ-efficiency of the beta-counter $(\varepsilon_1 + \varepsilon_2)$ is changed by the absorber to $(\varepsilon_1' + \varepsilon_2')$.

Another way of varying e_β is to alter the distance between the source and the beta-counter, when e_β will vary approximately as the solid angle between the source and the counter. But a plot of $(N_{\beta\gamma} + N_{\gamma\gamma})$ against $(N_\beta + N_\gamma \ln \beta)$ will not then give a straight line, because the γ-efficiency of the beta-counter will also vary with the solid angle. This may account for the variation in disintegration rate which has been observed in this laboratory using the conventional absorber method with varying geometrical arrangements. Lastly, it may be noted that measurements on sources of different strengths should not affect the slope of the line, provided that the γ-efficiency of the gamma-counter remains constant.

It is hoped to pursue this work on the standardization of cobalt-60, to investigate the method more fully, and to apply the method to other suitable isotopes.

This work has been carried out as part of the research programme of the National Physical Laboratory, and is published by permission of the Director of the Laboratory.

[1] Putman, J. L., "Beta- and Gamma-Ray Spectroscopy", 823 (North-Holland Pub. Co., London, 1955).
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EVIDENCE FOR A PROTEIN COMPONENT IN THE MIDDLE LAMELLA OF PLANT TISSUE: A POSSIBLE SITE FOR INDOLYLACETIC ACID ACTION

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WHILE looking for a method for separating root tips into single cells, I attempted to use calcium-binders, following Northcraft[1]. Similar methods have been used for separating animal tissue[2–4]. Northcraft used ammonium oxalate. The actions of ammonium oxalate, citrate and ethylene diaminetetraacetic acid were compared on the assumption that calcium stabilizes the intercellular cement, as cited by Bonner[5]; calcium pectate is found in the middle lamella. The stabilizing action of calcium might also be due to its decreasing the zeta-potential of the colloidal pectins. Zwilling[4] obtained separation in animal tissue within 20–30 min., using ethylene diaminetetraacetic acid. Chelating compounds having high affinity for calcium should, therefore, readily and quickly also separate plant tissue.

The following method was used. The 1-mm. root tips of Alaska pea seedlings grown for 45–50 hr. were used. The root tips were immersed in 1 ml. of the test solution in small vials, and at the appropriate times they were put on slides in a drop of distilled water. By tapping gently on the cover glass separation of root tips occurs. Under the microscope the percentage of separation may be estimated.

Some typical results of the first sets of experiments are shown in Figs. 1, 2 and 3. The following emerges:

(1) The degree of separation is dependent upon concentration up to 0·1 *M* ethylene diaminetetraacetic acid. At 0·01 *M* no separation was obtained, even at high temperatures. Above 0·1 *M* the extent of separation again decreases. This decrease is being

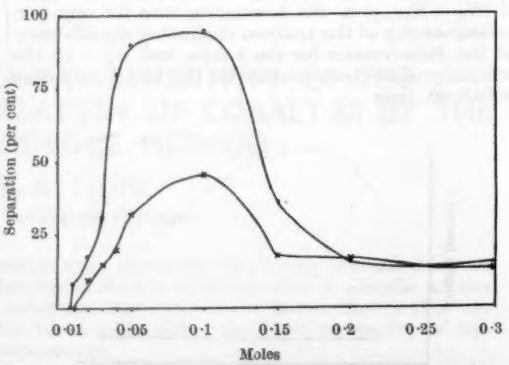

Fig. 1. Effect of ethylene diaminetetraacetic acid concentrations on separation of cells of Alaska pea root tips at 30° after different times. Solution *p*H 8·0. ×, 9-hr. treatment; ●, 16-hr. treatment

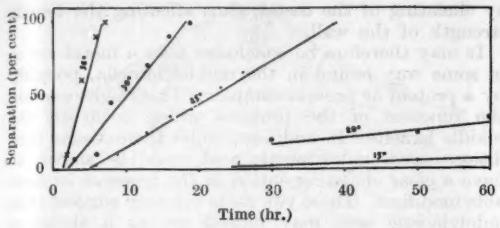

Fig. 2. Effect of temperature and time on the separation of cells of Alaska pea root tips. Ethylene diaminetetraacetic acid concentration 0·1 *M*. *p*H 8·0

investigated further. The active concentrations are very high as compared with those normally used in biological experiments and also as compared with those used for estimating calcium. (2) The rate of reaction is rather slow, compared with other known chelating reactions *in vitro* and *in vivo*. (3) The Q_{10} of the separation is very high, indicating a high 'energy of activation' which was calculated to be of the order of 35–40,000 cal. (4) Dependence on *p*H up to 11·0 is similar to, but not identical with, the calcium-chelating power of ethylene diaminetetraacetic acid as a function[6] of *p*H. This seems to indicate that an additional factor, apart from calcium chelating, is involved in the reaction.

These results cannot be interpreted as the mere removal of exchangeable calcium from the intercellular cement.

A working hypothesis was assumed as follows. The calcium ion is hidden by some structure and is therefore not accessible to ethylene diaminetetraacetic acid chelating action. This stabilized structure is part of the intercellular cement. Now, it seemed reasonable to assume that such a structure is part of a macromolecule existing in the middle lamella. It might be a pectic substance or other macromolecule that has not yet been identified in the middle lamella of plants, such as a protein or protein-containing complex. If such a macromolecule is involved, the use of the known agents breaking intramolecular links should modify the effect of ethylene diaminetetraacetic acid. Three substances were therefore selected. Urea, for breaking hydrogen bonds; thioglycolate, for breaking S—S bonds; and butanol, for breaking the hydrophobic linkages (Tables 1, 2 and 3).

Table 1. Separation of Cells in Alaska Pea Root Tips after Treatment with Ethylene Diaminetetraacetic Acid and Urea for 8 and 26 hr. Solution *p*H 8·0

Ethylene diaminetetraacetic acid concentration (mol.)		Per cent of cells separated		
		0	0·1	0·01
— Urea	After	—	10	—
+ Urea 0·125 *M*	8 hr.	0·0	80	—
— Urea	After	—	—	7
+ Urea 0·125 *M*	26 hr.	0·0	—	90

Table 2. Separation of Cells in Alaska Pea Root Tips after Treatment with Ethylene Diaminetetraacetic Acid and Thioglycolate. Solution *p*H 8·0

Concentration of ethylene diaminetetraacetic acid (mol.)	Per cent of cells separated after 10·5 hr.	
	No thioglycolate	Thioglycolate 1 per cent added
0·1	15	32
0·01	1	40
0·001	0	50
0·0001	0	4
0	0	0

Table 3. Separation of Cells in Alaska Pea Root Tips after Treatment with Ethylene Diaminetetraacetic Acid and *n*-Butanol for 19 hr. Solution *p*H 8·0. Temperature 25° C. The results are given as per cent of cells separated

Ethylene diaminetetraacetic acid 0·1 *M*	Ethylene diaminetetraacetic acid 0·1 *M* + 5 per cent butanol	Butanol 5 per cent
40	100	0

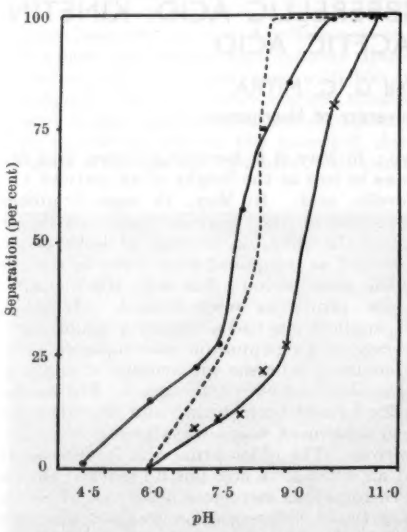

Fig. 3. Separation of cells of Alaska pea root tips at different *p*H values. Ethylene diaminetetraacetic acid concentration 0·1 *M*. Temperature 25° C. ×, 9-hr. treatment; ●, 26-hr. treatment; ----, chelating action of ethylene diaminetetraacetic acid for calcium at various *p*H (after Martell and Calvin, ref. 6)

The results of these experiments are in accord with the view that there is a protein component in the intercellular cement, as all the denaturing agents favoured the destruction of the intercellular cement. Particularly the action of thioglycolic acid supports this, if its action is indeed only on S—S bonds, which are known to occur in proteins.

Further evidence for this view was obtained from the action of proteolytic enzymes. Pepsin gave good separation of the cells, cathepsin *C* gave slight separation. Other enzymes, such as papain, trypsin and chymotrypsin, digest the whole tissue; no separation could be determined.

If the action of ethylene diaminetetraacetic acid is a chelating one, then other chelating compounds should also be effective in the presence of modifying agents such as thioglycolic acid (Table 4).

Table 4. Effect of Thioglycolate on the Effectiveness of various Chelating Agents in Separation of Cells in Alaska Pea Root Tips. Per cent Separation determined after 3 days. Temperature 25°

The results are given as per cent of cells separated

	Sodium diethylthiocarbamate *p*H 8·0	Phenanthroline *p*H 6·7	8-Hydroxyquinoline *p*H 6·0
No thioglycolate	0	15	0
Thioglycolate 1 per cent added	15	30	22·5

Table 5. Effect of 1 per cent Thioglycolate on Separation of Cells in Alaska Pea Root Tips by Indolylacetic Acid 10^{-4} *M*. Solution pH 11·0
The results are given as per cent of cells separated

24 hr.		7 days	
Without thioglycolate	With thioglycolate	Without thioglycolate	With thioglycolate
4	13	22	50

It will be seen that different compounds having different structures and having only chelating action in common work on the system in much the same way.

From the work of Heath and Clark[7,8], Weinstein *et al.*[9] and Bennet-Clark[10], it has been suggested that ethylene diaminetetraacetic acid and other chelating compounds have a growth-stimulating activity, and that the action of indolylacetic acid itself could be similarly interpreted as being due to chelating action. Indolylacetic acid, therefore, should also effect the separation of cells in the presence of a suitable protein-denaturing agent (Table 5).

If, to accept the old idea of Heyn, which has recently been recalled by Bennet-Clark[10] and by Bonner *et al.*[11], the effect of indolylacetic acid is to plasticize the cell wall or, in other words, to decrease the wall pressure of the cell, the following might be suggested. The 'wall pressure' can be seen as having two components—the pressure of the rest of the tissue and the tensile properties of the cell wall, that is, the wall pressure proper. Now the indolylacetic acid might act on the intercellular cement, and by partially loosening it, decrease the effect resulting from the pressure of the neighbouring cells. Alternatively, we may look upon the cement as part of the wall and the change of structure caused by chelating of the metal, thus affecting the tensile strength of the wall.

It may therefore be concluded that a metal ion is in some way bound in the middle lamella, possibly by a protein or protein complex. This might explain the function of the proteins which occur in the middle lamella. In addition, under the extreme conditions used, indolylacetic acid could be shown to have a clear chelating action in the presence of suitable modifiers. These two facts together suggest that indolylacetic acid may indeed act as a chelating agent. These facts would also be in agreement with the views of Burström[12], that ethylene diaminetetraacetic acid may act by competitive chelating action.

This paper is part of a Ph.D. thesis to be submitted to the Hebrew University, Jerusalem. I am indebted to Prof. M. Evenari, Dr. A. M. Mayer and Dr. A. Poljakoff-Mayber for their guidance, interest and encouragement during the time that this research was carried out.

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RESPONSES OF A FERN APEX TO GIBBERELLIC ACID, KINETIN AND α-NAPHTHALENEACETIC ACID

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THE reactivity of the shoot apex of *Dryopteris austriaca* (*D. aristata*) to the direct external applications of physiologically active substances[1,2] has been further investigated during treatments with gibberellic acid, kinetin and α-naphthaleneacetic acid. These substances have recently been shown to produce remarkable physiological and morphological changes when applied externally to leaves, etc., and in tissue culture[3–14].

Large, bare apices of *Dryopteris austriaca* were treated, as already described[1,2], for periods of about three weeks with aqueous solutions of gibberellic acid (100 mgm./l., 10 mgm./l.), kinetin (100 mgm./l., 10 mgm./l., 1 mgm./l., 0·1 mgm./l.), and α-naphthaleneacetic acid (1,000 mgm./l., 100 mgm./l., 10 mgm./l., 1 mgm./l.) and closely examined at frequent intervals. Control apices were similarly treated but with water throughout. The experiments with gibberellic acid and kinetin were undertaken in May and July, and those with α-naphthaleneacetic acid in January, February, May and July. (During the winter months the apical meristem of *Dryopteris* is inactive; in May it is becoming active, and in July it is more or less at the height of its activity.)

Gibberellic acid. In May, 10 mgm./l. gibberellic acid stimulated the growth and morphogenetic activity of the apex, an average of eight primordia being formed as compared with three in the control during the same period; but with 100 mgm./l. only three new primordia were formed. In July, 10 mgm./l. applications had a smaller stimulatory effect (an average of six primordia as compared with four in the control), whereas an average of eight primordia was obtained with 100 mgm./l. The number of primordia formed both during and after the gibberellic acid treatment was always greater than that in the controls. The older primordia in treated apices showed an increase in size but no marked elongation of the rhizomatous stem was observed. The normal course of tissue differentiation was not modified.

Kinetin. At 100 mgm./l. kinetin completely inhibited the growth of apices and eventually killed them. With 10 mgm./l. and 1 mgm./l. of kinetin, no new primordia were formed during a period of four

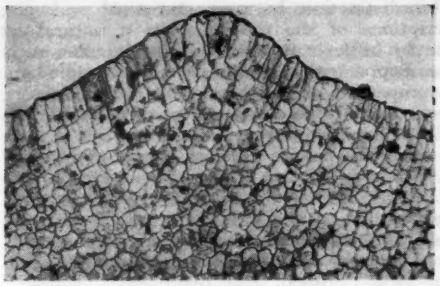

Fig. 1

weeks after the cessation of the treatment in either the May or July collections of apices. The apices underwent a considerable general increase in size, with formation of new scales around the base of the meristem and a conspicuous growth of the bases of the older scales which had been removed. At both concentrations, primordia 1 and 2 disappeared during the period of treatment and no organ re-formed in these sites when the treatment was discontinued. Later in the experiment, when the treatment had been discontinued, the growth of primordia 3 and 4 did not continue, and they were gradually transformed into flattish mounds. Primordia 5, 6, 7 and 8 likewise did not increase in size and were overtaken by the rapid growth of the surrounding tissues and old scale bases. Treatment with 0·1 mgm./l. kinetin slightly stimulated the formation of leaves, an average of six primordia being obtained as compared with four in the control. In microtome sections of treated apices it was seen that the normal histological organization had been profoundly affected and that tissue differentiation, for example, of the prevascular tissue had not occurred (Fig. 1). The general effect could be described as one in which cell division had been followed by a more or less uniform formation of parenchyma throughout the apical cone.

α-*Naphthaleneacetic acid*. Applications of 1,000 mgm./l. killed both winter and summer apices. In the winter experiments, treatments with 100 mgm./l. and 10 mgm./l. induced a very marked enlargement and saucer-shaped upgrowth of the subapical region (Fig. 2), indicating that in the so-called 'dormant' winter apex some growth potential still persists in this region. When treatment was discontinued, this subapical growth gradually became obscured by growth at the base of the apical cone. The latter, which includes the apical meristem, did not increase in size, but gradually became scaly right to the summit in some cases. In a February experiment, with controlled temperature (22–24° C.) and lighting (150-ft.-candles for 16 hr. per day), one or two new primordia were formed in some of the apices after the cessation of the treatments. A similar formation of primordia was observed in the controls.

Fig. 2

In the summer experiments with 100 mgm./l., no new primordia were formed during the period of treatment, and the subapical region did not form the characteristic upgrowth already described. The apical meristem appears to have retained its characteristic property of self-maintenance in the embryonic state and, when the treatment was discontinued, formed an average of three new primordia in three weeks. 10 mgm./l. did not seem to affect the growth of the meristem either during or after the treatment. Applications of 500 mgm./l. inhibited the growth of the meristem in some specimens while in others it stimulated growth in the subapical region, but did not induce the saucer-shaped upgrowth. Treatment with 1 mgm./l. was slightly stimulatory to the formation of leaves by the apex.

The results obtained with gibberellic acid are in general accord with previous findings with other plants, except that there was no notable elongation either of young primordia or of the stem. Apices collected in May and July responded differently to the two concentrations; such seasonal differences in apical reactivity have frequently been noted in this laboratory. In the experiments under consideration they are especially interesting in respect of the responses of winter and summer apices to different concentrations of α-naphthaleneacetic acid. The reactivity of different regions of the apex to α-naphthaleneacetic acid was also a notable feature and one which needs fuller investigation.

Kinetin is known to have both inhibitory effects[9,15] and stimulatory effects on cell division[5,6] and enlargement[7]. In *Dryopteris*, kinetin stimulates cell division and parenchymatous enlargement throughout the whole apical region. As a result, the apical organization is so modified that it no longer gives rise to the characteristic pattern of tissue differentiation that normally attends the inception and formation of organs.

These observations will be considered in greater detail elsewhere.

We are indebted to Mr. E. Ashby and Mr. G. Barker for the photographs.

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LETTERS TO THE EDITORS

The Editors do not hold themselves responsible for opinions expressed by their correspondents. No notice is taken of anonymous communications.

Irreversible Processes in Physical Theory

Prof. K. R. Popper[1], in several communications on the nature of irreversible processes, has formulated the idea that the irreversibility of a simple physical process arises from the causal impossibility of arranging for coherence between the independent oscillators required for the operation which is the inverse of that process.

Prof. E. L. Hill and A. Grünbaum[2] believe that the point raised is sufficiently important to deserve formulation as a general principle in physics. They suggest that the distinction between physically reversible and irreversible processes is parallel to that between closed and open systems. A closed system is quasi-periodic and causally reversible. An open system is aperiodic and irreversible. In particular this distinction requires that the process of radiant heat transfer in an infinite universe be just as irreversible on the quantum theory as on classical mechanics. The difficulty of this point has already been stressed by Einstein.

Popper casts doubt on the generality of the principle of Hill and Grünbaum. He claims, for example, that the system of the Sun and a comet moving on a hyperbolic orbit, while undoubtedly an open system, is definitely one we should like to regard as reversible. He then cites two further examples to elaborate his point with regard to the relation between reversibility and coherence. A spheroidal reflector with an oscillator at each conjugate focus constitutes a reversible system. A similar system but with a scattering instead of a reflecting surface, even if perfectly externally insulated, does not. This, says Popper, arises from the impossibility of arranging for coherence between the oscillators on the scattering surface by means of any centrally directed mechanism, thereby claiming that his principle of coherence is of more general validity than that of Hill and Grünbaum.

There is, however, an alternative way of describing the distinction between causally reversible and causally irreversible processes and one which I claim is of higher generality than that advanced by Popper.

In the description of a mechanical system by classical mechanics the parameters used, namely, the co-ordinates and momentum, give a complete description of the path of every particle of the system, and the reversibility of these paths is ensured by the ordinary laws of motion which are invariant with respect to change from $+t$ to $-t$. When, however, we come to describe a system by means of parameters which have only a statistical significance, a complete description of the path of every particle concerned is no longer obtained. The inverse process is then not necessarily a microscopic copy on a reversed time-scale of the direct process. The inverse process no longer completely undoes the action of the direct and the change is essentially irreversible. Thus, as I have pointed out elsewhere[3], a pure radiative process, with the optical paths of all rays concerned completely determined, is reversible. A conductive process is described by such statistical terms as mean free paths or mean collision frequencies and is accordingly irreversible.

The distinction between statistical and precise descriptions of the process gives a natural interpretation of the examples raised by the other authors. Thus, Popper's second example is irreversible because the properties of the oscillators on the absorbing screen can only be described by statistical qualities. His first example is reversible because it is possible to define the precise path of every quantum from one oscillator to the other. The addition of any scattering material, for example, smoke, within the reflecting body would diffuse the optical paths which would then only have statistical significance and the whole process of energy exchange would become irreversible. Again, the distinction between the description of divergent radiation on the wave and quantum theories disappears in this new view. Both processes are thermodynamically equivalent; one because the intensity of the radiation is progressively decreased and the other because the probability of finding a quantum in unit volume of space decreases at the same rate.

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Prof. R. C. L. Bosworth's view, if I understand him properly, is that held by most physicists since the time of Boltzmann. It has become famous through Eddington's metaphor of the arrow of time. It consists of the following two theses: (1) Every non-statistical or 'classical' mechanical process is reversible; this, to quote Prof. Bosworth, "is ensured by the ordinary laws of motion which are invariant with respect to change from $+t$ to $-t$"; (2) irreversibility enters into physics "by means of parameters which have only a statistical significance"; or in Eddington's way of putting it, the increase of entropy (or of statistical disorder) determines the "arrow of time".

It was precisely this position, so ably restated in Prof. Bosworth's communication, which I briefly sketched at the beginning of my communication "The Arrow of Time"[1], which started this correspondence, and which I tried to criticize in that letter by giving a counter example to thesis (1). I did not discuss my reasons for disbelieving thesis (2).

Prof. Bosworth questions my counter example in its optical form in which it appears in my reply to the communication by Profs. Hill and Grünbaum[2], by asserting that "the properties of the oscillators on the absorbing screen can only be described by statistical qualities". But there is surely some confusion here. We may assume—and this becomes clear if we revert to my original example of a water wave—that all the elementary processes are purely mechanical and *prima facie* deterministic (admittedly an over-simplification): only then does it become quite clear that the process in which we are interested is theoretically fully reversible, since we may in fact construct a large number of oscillators, operating coherently, which actually reverse it. But if we look more closely at the way in which these oscillators are controlled so as to make them cohere, we find that they must, essentially, be controlled from one centre from which some signal—for example, a light

signal—must run to the places where the oscillators are situated.

Thus although the elementary classical physical processes involved in an expanding wave are reversible, we find that in order to obtain the reversion, that is, the contracting wave, we always need a wave that spreads from a centre, or something equivalent to it. If not steered by an expanding wave, the contracting wave, though not in itself physically impossible, would nevertheless have the character of a physical miracle: it would be like a conspiracy, undertaken by many people, each carefully acting so as to support all the others, but without any previous arrangement, or anything like a prepared plan.

This explains why, although theoretically possible, spontaneous contracting waves do not in fact occur: they are theoretically possible, but causally, and therefore physically, impossible.

Prof. Bosworth's analyses of my examples are inadequate: in the reversible case of the ellipsoid mirror (called by Prof. Bosworth my first example) contracting waves never occur unless they are excited by an expanding wave; and the example was designed to show, among other things, that even perfect reversibility is not yet causal reversibility.

Of course all these experiments are idealizations, and, in reality, there would be in all these cases an increase of entropy. So much is common ground. Also, in explaining any concrete mass phenomena, we have of course to use statistical methods.

Although my whole analysis was only intended to throw light on (1) by showing that even elementary classical processes may be in an important sense physically irreversible, I may perhaps say, very briefly, why I distrust (2). The suggestion has been made (first by Boltzmann himself) that the arrow of time is, either by its very nature, or by definition, connected with the increase in entropy; so that entropy cannot decrease in time because a decrease would mean a reversal of its arrow, and therefore an increase relative to the reversed arrow. Much as I admire the boldness of this idea, I think that it is absurd, especially in view of the undeniable fact that thermodynamic fluctuations do exist. One would have to assert that, within the spatial region of the fluctuation, all clocks run backwards if seen from outside that region. But this assertion would destroy that very system of dynamics on which the statistical theory is founded. (Moreover, most clocks are non-entropic systems, in the sense that their heat production, far from being essential to their function, is inimical to it.)

I do not believe that Boltzmann would have made his suggestion after 1905, when fluctuations, previously considered no more than mathematically calculable near-impossibilities, suddenly became the strongest evidence in favour of the physical reality of molecules[3]. (I am alluding to Einstein's theory of Brownian motion[4].) As it is, a statistical theory of the arrow of time seems to me unacceptable. The purpose of my first communication was to propose an example of a non-statistical process such that no physicist who finds a film strip of this particular process would need to doubt where the film begins and where it ends: he could determine its arrow of time.

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Life-time of an Artificial Earth Satellite

Fejer[1] and Scott[2] have given simple methods for estimating the remaining life-time of an artificial Earth satellite. Their methods do not apply when the eccentricity is small, and it is not immediately clear why the estimates are so good (cf. particularly Scott's data for the descent of the rocket of 1957 Alpha Satellite). Nevertheless, it has been found possible to show that their formulæ are accurate so long as the eccentricity is not too large.

A strict application of first-order perturbation theory to the motion of a satellite subjected to a purely axial force with constant drag coefficient shows that the rate of descent of apogee per revolution is given by

$$\frac{dH_a}{dn} = -\delta . H^{a2} \int_{-\pi}^{\pi} (1 + \cos\theta) \frac{\sqrt{1 + 2e\cos\theta + e^2}}{(1 + e\cos\theta)^2} \rho(\theta)\, d\theta \quad (1)$$

where H_a is the height of apogee above the centre of the Earth, $\delta = SC_D/m$ in the usual notation, θ is the anomaly, e the eccentricity, n the number of revolutions, and ρ the density. The rate of descent of perigee dH_p/dn is given by a very similar formula. If e is small enough and if a simple exponential atmosphere is an adequate model, the integral (1) may be evaluated exactly (as was noted by Scott) to give

$$\frac{dH_a}{dn} = -2\pi\delta H_a^2 \rho_o \exp\{\beta(H_o - H) - \beta h_o\}[I_0(\beta h) + I_1(\beta h)] \quad (2)$$

where $H = \frac{1}{2}(H_a + H_p)$, $h = \frac{1}{2}(H_a - H_p)$, ρ_o is the density at height H_{po}, β the lapse-rate of the atmosphere, and a suffix o denotes an initial value. I_0 and I_1 are Bessel functions of the first kind and imaginary argument. The formulæ given by Fejer and Scott are the asymptotic forms of this equation for large βh. Thus the Scott–Fejer analysis seems to require that e should be both large and small.

Equation 2 and the very similar formula for dH_p/dn may be regarded as equations of motion for H_a and H_p (or H and h) as functions of n. Since βH_{po} is large, they may be integrated when e is small, to give

$$\exp\{\beta(H_o - H)\} = \frac{h_o I_1(\beta h_o)}{h I_1(\beta h)} \quad (3)$$

and

$$h_o^2 - h^2 = 4\pi\delta H_o^2 \rho_o . h_o \exp(-\beta h_o) . I_1(\beta h_o) . n \quad (4)$$

From equation 4 it follows that:

$$e_o^2 - e^2 = 4\pi\delta\rho_o . h_o \exp(-\beta h_o) . I_1(\beta h_o) . n \quad (5)$$

The linear variation of e^2 with n also follows from the analysis of Fejer and Scott. The fundamental reason for the success of their work is that the relation is in fact valid down to $e = 0$. From it, we may deduce that the remaining life-time of the satellite is

$$t_o = \tfrac{1}{2} e_o \frac{T_o}{(-dT/dt)_o} \quad (6)$$

T_o being the initial period, and $(-dT/dt)_o$ the initial rate of decrease of the period; if this decrease is

measured in seconds per day, t_0 is immediately expressed in days, which is convenient. Scott's equation 1 agrees with my equation 6 to order e_0, while Fejer's equation 7 is a direct consequence of my equation 4.

The relative error in equation 6 is believed to be of order e_0. A correction of this form would be of some significance for Satellite 1957 Beta, with its long life and high initial eccentricity, and an attempt is therefore being made to evaluate it.

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Remanent Magnetization of Ancient Bricks

It has recently been suggested that, in the values of remanent magnetization of clay bricks fired in ancient times, there exists evidence of a secular decrease in the strength of the Earth's magnetic field[1,2]. It has been further suggested that the effect of such a change in magnetic field on the intensity of cosmic radiation incident upon the Earth would tend to falsify radiocarbon dating procedures based on an assumed constant intensity of cosmic radiation[3].

The conclusions based on the magnetization of ancient bricks have presupposed that this magnetization is proportional to the intensity of the Earth's magnetic field at the time the bricks received their original firing, provided that no high-temperature firings have intervened. In the procedure of E. Thellier[1,2,4], the magnetization, M, of the brick, as found, is measured at a standard temperature (60° C.). The brick is then fired at successively higher temperatures to 670° C., apparently in an inert atmosphere[2], and then allowed to cool from 670° to 60° in the present Earth's field, H'. The resulting magnetization of the brick, M', is then measured. The value of the Earth's field, H, at the time of the original firing, is assumed to be related to the above values by the equation :

$$H = H' M/M'$$

However, it has been shown in an earlier paper of E. Thellier[5] that the specific magnetization, σ, acquired by a given baked clay, when fired at 670° in a constant magnetic field of 13 gauss, can increase by a factor of 200 when the firing atmosphere is changed from nitrogen to illuminating gas. (Cf. Table 1, taken from ref. 5. T is the maximum firing temperature.)

It is proposed, therefore, that the apparently high value of remanent magnetization of ancient bricks can have been the result of the use of fuel-heated kilns during their original firing. The presence of incompletely burned products of combustion in such kilns could provide a reducing atmosphere comparable in effect to illuminating gas. This would tend to cause a higher value of magnetization to exist in the brick after cooling than would result from firing in an inert atmosphere.

Table 1. Specific Magnetization of Fired Clay ($\sigma \times 10^4$)

Temperature (° C.)	400°	580°	670°	870°
Oxygen	2·0	3·6	4·8	6·5
Nitrogen	2·6	4·5	10·0	570·0
Illuminating gas	520·0	4,750·0	2,000·0	36·0

If the possibility of the occurrence of such an effect has not been taken into account in the measurements of 'fossil' magnetization of ancient clays, the results may well lead to the deduction of an excessively high value of the magnetic field of the Earth at the time of the original firing.

We are indebted to Dr. L. S. Cressman and Mr. Thomas Newman of the University of Oregon for directing our attention to the measurements for dating by clay brick magnetization.

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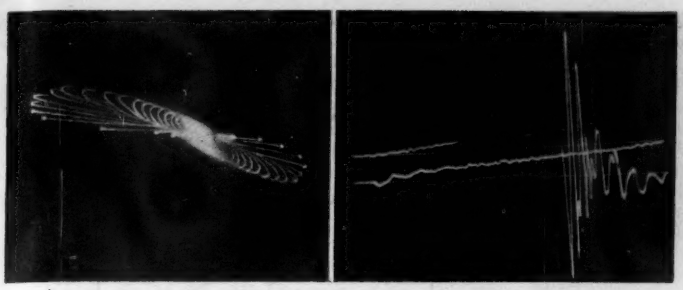
Abnormal Polarization of the Atmospheric Pulses reflected successively from the Ionosphere

While engaged in the determination of the direction of arrival of atmospherics by the conventional cathode-ray tube direction-finder, it was observed that, on occasions, for atmospherics of comparatively distant origin, there were elliptic patterns of gradually decreasing eccentricity and tilt-angle along with one or more linear responses, although when tested with the audio-frequency oscillations from a signal generator fed into the aerial system of the cathode-ray tube direction-finder, the fluorescent screen exhibited only the usual linear responses. It appears certain that these elliptic patterns are evidences of abnormal polarization of the atmospheric pulses reflected successively from the ionosphere, the linear responses being due to the direct (or ground-wave) atmospheric pulses. More than forty oscillograms showing the elliptic patterns and lines were recorded during the monsoon months of 1955. Usually the tilt-angle of the ellipses was found to decrease with the increasing order of ionospheric reflexion. A reversal in the direction of precession was noticed in few cases.

A typical record of the precessing ellipses with five or six straight lines bunched within a small angle along with the record of the wave-form of the atmospheric pulses simultaneously taken with the automatic atmospheric recorder[1] is shown in Fig. 1. The wave-form record reveals quasi-periodic pulses followed by a series of successive ionospheric reflexions. The distance of the lightning discharge yielding these records, as calculated from the observed time-interval between the successive reflexions, is about 500 km. The tilt-angle of the elliptic patterns was found to decrease from about 176° to about 167° and the phase-difference between the normal and abnormal components, as calculated from the observed ellipses, was found to increase steadily from about 40° to about 67° after six successive reflexions from the ionosphere.

It is suggested that the several straight lines, bunched within a small angle, were due to the



(a) (b)

Fig. 1. Oscillograms of (a) the elliptical patterns with gradually varying eccentricity and tilt-angle and (b) the corresponding wave-form recorded simultaneously with the automatic atmospherics recorder. Date, October 10, 1955. Time, evening

radiation which originated at the branching points of a long horizontal or slightly inclined lightning discharge from one cloud to another and reached the direction-finder directly. The quasi-periodic pulses observed in the wave-form pattern were due to the current variations at the branching points, and they were followed by the different orders of ionospheric reflexion. The horizontal length of the lightning channel responsible for the oscillograms, shown in Fig. 1, has been calculated and found to be about 7 km. This seems to be consistent with the observations of Shipley[2], who reported long horizontal channels along the lower edge of the cloud.

The possibility that abnormal polarization might be shown by the atmospheric pulses due to the zig-zag path of the lightning discharge was first pointed out by Adcock and Clarke[3]. The observed regular pattern of the precessing ellipses with continuously changing eccentricity and tilt-angle, however, strongly suggests that the effect of the random zig-zag path of the lightning discharge cannot be appreciable.

The experimental work was carried by Dr. B. A. P. Tantry and Mr. R. S. Srivastava under my guidance, as a part of the programme of a research project sponsored by the Council of Scientific and Industrial Research, New Delhi, to whom our thanks are due. A detailed paper will be published elsewhere.

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Temperature Coefficient of Ultrasonic Absorption in Organic Liquids

FROM the data heretofore available in regard to the observed ultrasonic absorption and its ratio to the coefficient calculated on the basis of Stokes's and Kirchhoff's theories, the various organic liquids have been broadly classified[1,2] into four groups. Confining our attention to temperature variation alone, we find that, according to this classification, members of group *A*, consisting of non-associated polyatomic liquids, are expected to show a positive temperature coefficient of absorption, whereas the liquids in the other groups are expected to show a negative temperature coefficient. It may be remarked, however, that the grouping and also the temperature coefficient assigned to each group are on an empirical basis.

In the course of an exhaustive investigation carried out in this laboratory on the temperature variation of absorption coefficient in a large number of liquids, including several homologous series, it has been noticed that some liquids which fall in group *A* show a negative temperature coefficient of absorption instead of positive as expected. Aniline, hexadecane, 1-dodecene and amyl acetate are some of the liquids showing this behaviour. Further, in some of the homologous series such as the saturated hydrocarbons and 1-olefines it is seen that the temperature coefficient is positive for the lower members but becomes negative for the higher members, passing through a stage where the absorption coefficient is practically independent of temperature over the range of observation (15–60° C.), as, for example, for *n*-decane in the case of the saturated hydrocarbons and for 1-nonene in the case of 1-olefines. Some of the measurements are given in Table 1.

Liquid / Temp. (° C.)	$\alpha/\nu^2 \times 10^{17}$ (at 21 Mc./s.)					
	20°	25°	30°	35°	40°	45°
Aniline	66·7	60·0	54·5	51·5	49·0	48·2
Amyl acetate	86·0	84·3	82·9	81·5	79·8	79·2
n-Hexane	56·9	60·6	63·6	66·3	68·9	71·4
n-Decane	56·7	56·7	56·7	56·7	56·7	56·7
n-Hexadecane	97·8	95·4	91·9	88·9	86·2	83·8
1-Octene	57·6	57·4	57·4	57·5	57·9	58·6
1-Decene	51·4	49·5	47·8	46·3	44·5	44·2
1-Dodecene	72·6	68·0	65·5	62·3	59·5	57·1

The object of this communication is to report on the anomalies between our observations and the classification and to direct attention to the inadequacy of the present theories to explain them. The observation regarding negative temperature coefficient of absorption in some of the above-mentioned liquids, and also the reversal of temperature coefficient from the lower to the higher members of the homologous series, is not consistent with the present classification of liquids according to their sound absorption, nor is it accounted for by the Stokes's and Kirchhoff's theory of classical absorption.

By comparing the above observed values of sound absorption with those calculated according to the classical theory it is seen that in every case the measured value is larger. As is well known no theory is able uniquely to explain the excess absorption in all the liquids. The various theories[3,4] advanced so far are based on thermal relaxation, structural relaxation and bulk viscosity. A formula[5] lately given correlates the observed absorption to the classical absorption in terms of a function of γ, the ratio of specific heats, as

$$\frac{\alpha_{obs.}}{\nu^2} = \left(\frac{2/3}{5/3-\gamma}\right)^4 \left\{\frac{8\pi^2}{3v^3\rho}\left(\eta_l + \frac{3}{4}\frac{\gamma-1}{c_p}K_{...}\right)\right\}$$

It may be remarked here that the existing classification of liquids into various groups is of an *ad hoc* nature and has to be examined closely.

The observation that some of the measured liquids do not fit into the present classification and that in a homologous series the temperature coefficient of absorption which is positive for the lower members changes to negative for higher members is being examined in terms of the available theories.

Detailed discussion of the topic will be published elsewhere.

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Influence of Moisture upon the Dielectric Properties of Solid Powders dispersed in Liquid Paraffin

It is known that various colloidally or coarsely dispersed systems show anomalous dielectric behaviour. Fricke[1] reported that an aqueous dispersion of solid particles or macromolecules showed a striking dielectric dispersion in the audio- and radio-frequency ranges, and he ascribed these effects to the polarization of oriented water molecules bound loosely at or near the surface of the dispersed phase. But the exact nature of the effect has not been established.

We have measured the dielectric constant and loss, ε' and ε'', of various suspensions or pastes consisting of inorganic or organic powders dispersed in liquid paraffin, over a frequency-range of 0·5–3,000 kc./s. by means of a conductance-capacitance bridge. As is shown in Fig. 1, pronounced dispersion was found with suspensions of titanium oxide in both its crystalline forms, anatase and rutile. The titanium oxide powders had been air-dried and contained about 0·7 per cent moisture by weight; the content of the powder was about 13 per cent by volume, and the dispersed systems remained sufficiently stable during measurements.

The effect was more pronounced with anatase than with rutile. For a suspension of rutile, the dependence upon frequency of ε' and ε'' is expressed precisely by the following relations, which have been given for insulating materials by Schweidler[2]:

$$\varepsilon' - \varepsilon = A \cot(\pi m/2)\omega^{-m} \quad (1)$$

$$\varepsilon'' = A\omega^{-m} \quad (2)$$

$$\varepsilon''/(\varepsilon' - \varepsilon_\infty) = \tan(\pi m/2) \quad (3)$$

where ε_∞ is the limiting value of ε' at high frequencies, A and m the empirical constants characteristic of the dispersed systems, and ω the angular frequency. Fig. 2 shows the linear relation between ε' and ε'' which can be expected from relation 3. In the case of anatase, the experimental results showed some deviation from these relations. Similar dielectric behaviour was observed also for a suspension of calcium carbonate powder.

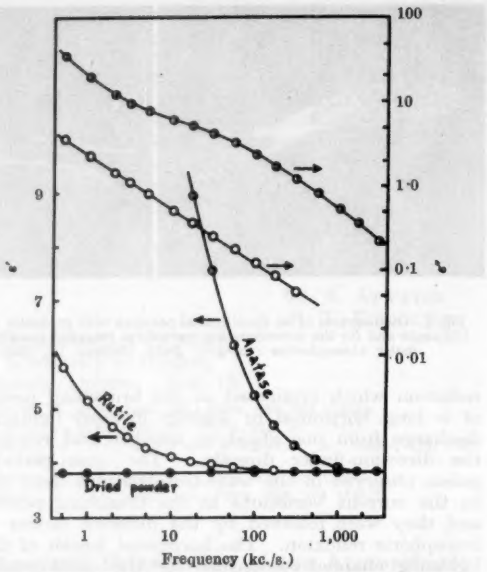

Fig. 1. Dependence upon frequency of ε' and ε'' for dispersed systems of titanium oxide (○ rutile, ◑ anatase and ● dried specimens) in liquid paraffin at 20° C. Particle size, moisture content and volume concentration of powders are about 0·4 micron, 0·7 per cent and 13 per cent respectively for both types of titanium oxide

However, this dependence upon frequency disappeared completely when the powder was dried at 150° C. for 3 hr., and the dielectric constant ε' remained constant over the range of frequency used, giving the value of ε_∞ defined above, whereas ε'' was too small to be observed. Thus it is evident that the dielectric dispersion of the suspension mentioned above arises from the moisture adsorbed by the powder. This interfacial effect may be essentially the same as that pointed out for aqueous dispersions by Fricke.

In the cases of more hygroscopic powders, for example, starch and bentonite, the dependence upon frequency of ε' and ε'' was more complicated. It seems that, in these cases, anomalous dispersions of the type mentioned above and of the Debye type due to the polar groups overlap. The values of ε' for the powders of copper, aluminium and sulphur have been shown to be independent of the frequency even for air-dried specimens which contained

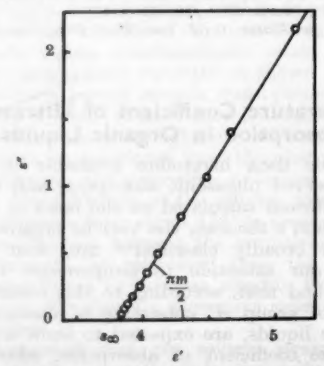

Fig. 2. Plots of ε'' versus ε' for the rutile suspension; $m = 0{\cdot}644$

appreciable moisture of more than 0·1 per cent by weight.

A more complete account of this work will be published elsewhere. We thank Prof. E. Suito and Prof. S. Ono for gifts of samples.

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Electron Micrographs of a Crystalline Lattice Structure in Yolk Platelets of the Amphibian Embryo

YOLK platelets are one of the cytoplasmic inclusions characteristic of embryonic cells, and constitute the most important reserve material for these cells. A weak birefringence has been reported from fresh or fixed yolk platelets of various amphibian embryos[1]. However, no further information is available concerning the physical organization of these inclusions. In the course of our electron microscopic studies of embryonic cells, regular fringe systems have been found over the whole area of yolk platelets, indicating a crystalline structure of the yolk matrix.

Small pieces of the ectoderm were isolated from embryos of the newt, *Triturus pyrrhogaster*, at various stages[2] from the gastrula (stage 11) to the tail bud (stage 26). 1 per cent osmium tetroxide in Holtfreter solution buffered with veronal acetate at *p*H 7·8 was used as the fixative. After fixation at 4° C. for 2 hr. and subsequent washing in Holtfreter solution for a few minutes, the fixed material was dehydrated in a series of graded ethanols, and embedded in a mixture of methacrylates. Sections were made on an ultramicrotome at a thickness less than 50 mμ, picked up on a collodion-coated grid and examined with the Hitachi electron microscope *HU*–10 at 75 kV.

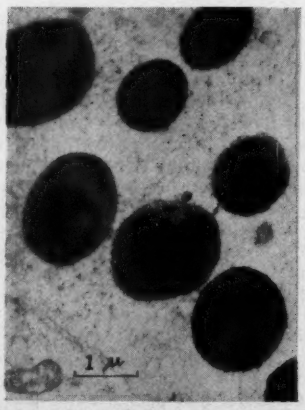

Fig. 1. Electron micrograph showing a group of yolk platelets scattered in the cytoplasm of an ectoderm cell of a late gastrula. One mitochondrion and the cell boundary are visible (below, left). × 10,000

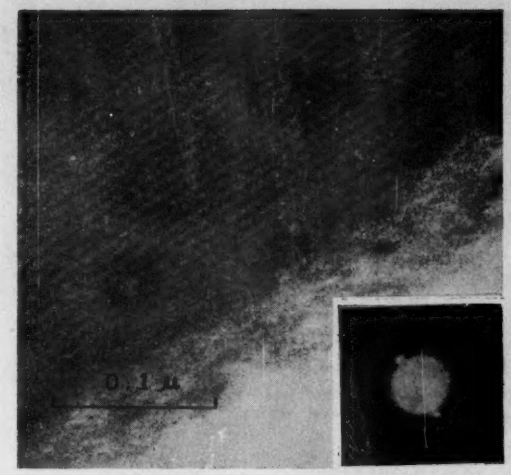

Fig. 2. High-power electron micrograph and selected area diffraction diagram (inset) of a yolk platelet. The granular layer attached to the surface of the yolk platelet is visible below in the main picture. Note the fringe system in the yolk platelet proper. × 250,000

In the embryonic ectoderm cells, the cytoplasm is moderately laden with yolk platelets. Under the electron microscope, the platelets appear as sharply outlined dark ovals varying from 0·3 to 6μ in diameter. The platelets are surrounded by a dense layer of fine granules of about 100 A. (Fig. 1). This layer disappears during the tail-bud stage, when significant changes take place on the surface of the yolk platelets[3].

Fig. 2 shows a high-magnification electron micrograph of the yolk platelets at the neurula stage, in which a system of fringes with spacing of 70 A. is evident. Careful observation reveals another system of weak fringes which is oblique to the main system. Similar fringe systems were observed in yolk platelets from the gastrula to the tail-bud stage. It should be noted that these fringes are always straight, and no dislocation has so far been observed in them. The regular arrangement of fringe systems proves that the matrix of yolk platelets is crystalline.

In the electron diffraction pattern of the yolk platelet, two pairs of spots can be observed (inset, Fig. 2). One pair is fairly strong and the other very weak. The presence of two pairs of spots would be expected from the existence of two systems of fringes intersecting each other in the electron micrograph. The distance between these spots corresponds to a fringe-spacing of 72 A. in both pairs. This spacing is in good agreement with that measured directly in the electron micrograph.

During the period of yolk resorption, the yolk platelets were found to split into parallel disks and rodlets[4]. A similar phenomenon of lamellar cleavage was observed also *in vitro* when the yolk platelets were exposed to weak acid or alkaline solutions[5]. These observations support the view that the crystalline structure seen in the fixed sections is not caused by the fixative but exists in the living cells.

We thank Profs. T. Yamada, R. Uyeda and Y. Sakaki for valuable suggestions, and the staff of the Hitachi Central Research Laboratory for generous assistance. The investigation was partly supported by grants from the Rockefeller Foundation

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An Instrument for measuring Evaporation from Natural Surfaces

The need has long existed for a direct method of measuring evaporation from natural surfaces for application in a wide range of problems connected with agricultural science, hydrological engineering and the like. The only method hitherto generally available has been that of isolating and weighing a portion of the evaporating body—a procedure which is cumbersome and often seriously interferes with the process it purports to measure. Reliance has therefore often been placed on formulæ, usually of doubtful validity, relating evaporation with other meteorological variables.

We have constructed prototype equipment which permits the automatic, direct determination of natural evaporation by equating it to the vertical turbulent flux of water vapour in the layers of air near the ground. The validity of this equation demands that there be no vertical transport of water vapour by mean vertical motion nor accumulation of water vapour between the ground, and the level of the instrument, and that a measurement of turbulent flux made at a point be representative of an area. Practically, this means that the area must be one of good horizontal uniformity and that the measurement must be made within a few metres of the ground. Providing that these conditions are fulfilled, there is no restriction on the nature of the surface. Measurements can then be made over, say, a growing crop or a free water surface with equal facility, and the method is directly applicable to problems such as those indicated above.

The turbulent flux of water vapour is given by the covariance between (1) the product of air density (ρ) and the vertical velocity component (w) and (2) the specific humidity (q). The principle involved was first used by Swinbank[1], who recorded fluctuations of ρw and of q and afterwards calculated the required covariance, $\overline{(\rho w)'q'}$ (where the primes indicate deviations from means and the bar an averaging with respect to time), at first by hand methods and later on a differential analyser. This method, though establishing the usefulness of the principle, is much too laborious for routine use.

The instrument which we have developed provides continuous electrical signals proportional to ρw and q, and performs the necessary mathematical operations of multiplication and integration automatically. Since the mean values of ρw and q cannot be known in advance, it is necessary to evaluate the required covariance in the form

$$\overline{(\rho w)'q'} = \overline{(\rho w - a)(q - b)} - (\overline{\rho w - a}).(\overline{q - b})$$

where a and b are arbitrary zeros from which ρw and q are measured. A block diagram of the instrumental arrangement is shown in Fig. 1.

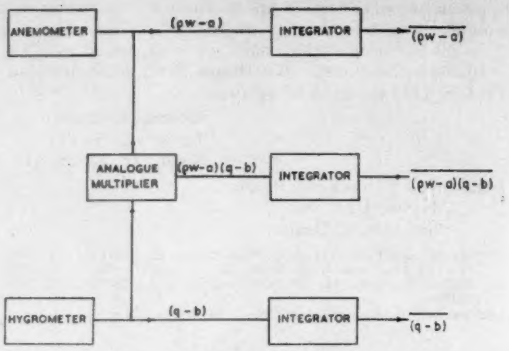

Fig. 1. Block diagram of evaporimeter

A height of 1·5 m. above the ground has been found suitable for mounting the sensing devices, and, at this height, response times of about 0·3 sec. are needed in the anemometer, hygrometer and multiplier in order to take account of the contribution to the flux made by the smallest eddies (1–2 sec. period) which are important at that height. A minimum period of observation of 5 min. is needed to accommodate the largest contributing eddies.

The anemometer, which is of an entirely new design[2], is linear and has a sufficiently rapid response. It depends in its operation on measuring the temperature in the wake of a heated wire by resistance-bridge thermometry and, by means of an amplifier feedback circuit controlling the input voltage to the bridge, this voltage can be made linearly dependent upon the product of ρ and the velocity component perpendicular to the instrument. Two such anemometers are arranged at equal and opposite angles to the horizontal, and the difference of their outputs is obtained as a measure of ρw.

The hygrometer is based on fine wire wet- and dry-bulb resistance thermometers of a design due to McIlroy[3]. These are constructed of fine nickel wire (0·0005 in. diam.) in order to obtain a sufficiently small response time, and are adjusted to a standard calibration of 900 ohms at 0° C. and 1,020 ohms at 30° C. A separate bridge network is used for each thermometer, the wet-bulb bridge being included in an electronic feedback circuit which simulates the saturated vapour pressure/temperature relationship of water[4]. The outputs of the two bridges are then combined in accordance with the psychrometric equation and an output signal linear with respect to $(q - b)$ is obtained.

Analogue multiplication is achieved by the quarter-square technique[5] making use of the identity $mn = \frac{1}{4}\{(m + n)^2 - (m - n)^2\}$. The squaring circuits each consist of a chain of ten diodes acting as function generators, four such chains being required for four-quadrant multiplication. Additions and subtractions are performed according to standard computing techniques using operational amplifiers.

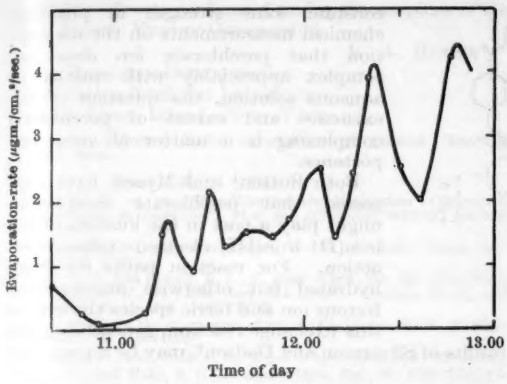

Fig. 2. Observed values of evaporation-rate, September 24, 1957

The integrators are of the electromagnetic type suggested by Gilbert[6], but with considerable modification of the original design to obtain greater accuracy and ruggedness. Special circuit arrangements have also been developed so that the integrator can accept currents of either sign. An important feature of this type of integrator in the present application is that it has no zero drift. A full description will be published by one of us (A. J. D.) elsewhere. The final results of the integrations appear on counters from which the total evaporation over the observation period is easily computed.

The complete equipment has been tested in the field, and some checks against Swinbank's method have been carried out with satisfactory results. A typical series of measurements is shown in Fig. 2, in which are plotted the results of a number of five minute runs. The increase in evaporation during the morning is evident, and the results demonstrate the inherent variability of the evaporation as a result of chance fluctuations in wind-speed, incoming radiation and other such factors.

Further field trials are now in progress, and development work is proceeding. Full instrumental details and the results of further observations will be published elsewhere.

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Lamellar Compound of Sodium with Graphite

It is well known that graphite forms lamellar compounds with potassium, rubidium and cæsium[1,2], but hitherto it has seemed likely, both on experimental[1-3] and theoretical[4] grounds, that sodium does not behave in this way. Recent work at Harwell reported here has, however, provided evidence for the existence of a lamellar compound of sodium with graphite.

The compound was prepared very simply in a helium atmosphere (less than 20 p.p.m. oxygen) in a 'glove-box'. Powdered graphite was heated with about 3 per cent sodium (commercial purity, filtered through sintered glass at about 150° C.) at about 400° C. for 1 hr. with continuous stirring. The conditions are not at all critical, since the compound has been formed with heating times between ¼ hr. and 6 hr. and at temperatures between 120° and 500° C. (although mixing is bad below about 200° C.). The product, virtually pure sodium-graphite compound, is very similar in appearance to the parent graphite but has a deep violet tinge. The analogous potassium, rubidium and cæsium compounds have similar strong colours; for example, C_8K is copper-coloured.

The main evidence for the formation of a compound is obtained from X-ray diffraction (compare the typical photographs in Fig. 1,*a* and *b*). We have found that the product made from a highly crystalline graphite, such as purified natural graphite, gives the most sharply defined X-ray diffraction pattern, but that the same product is formed from less-crystalline materials, for example, reactor-grade graphite; the definition is also improved by prolonged heating at about 400° C.

The formation of a compound is indicated in two ways. First, several diffraction lines which are strongly characteristic of graphite (for example, the $00\bar{0}2$ and $00\bar{0}4$ lines) are absent from the diffraction photograph of the product; as little as 5 per cent free graphite should be detectable in this way. Secondly, the product has a definite composition. Thus if excess of either sodium or graphite was used in the preparation, the excess was revealed roughly quantitatively in the photograph as unreacted material. The composition was found by this method to be between $C_{60}Na$ and $C_{68}Na$—probably $C_{64}Na$.

The change in structure thus indicated cannot be attributed to the formation of the potassium-graphite compounds since the potassium content of the sodium was certainly less than 0·1 per cent.

These diffraction results seem to fit best a structure in which sodium atoms lie in interstitial planes separated by eight carbon layers (that is, a lamellar compound) as indicated in Fig. 2,*a*. At every eighth layer, therefore, the spacing is increased in this way from 3·35 A. to about 4·6 A., which is consistent with

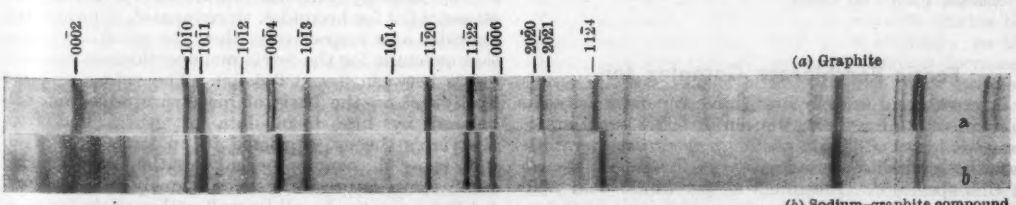

Fig. 1

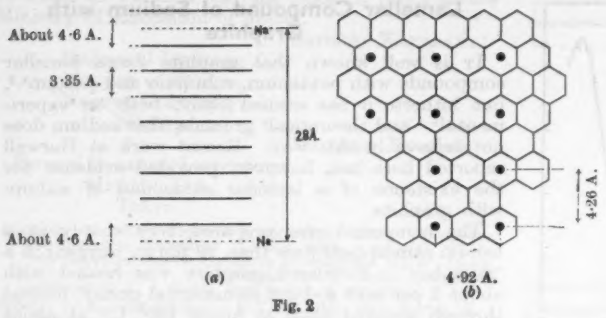

Fig. 2

the atomic diameters of sodium and carbon. The unit-cell dimension in the *c* direction then becomes 28 A. (or a simple multiple of this) and the average layer spacing is now 3·50 A., that is, 5 per cent greater than in the parent graphite. Graphite artefacts swell when immersed in liquid sodium, but only by about 0·5–1·0 per cent. The smaller magnitude of this swelling compared with the increase in the *c* spacing is due mainly to the fact that the crystallites in an artefact are anisotropically oriented. Since it is possible to estimate the degree of this anisotropy from the thermal expansion, we have been able to relate, at least approximately, the swelling to the change in the *c* spacing.

The detailed analysis of the X-ray photographs is not yet complete. At present, therefore, it is possible to make only a tentative suggestion for the arrangement of the sodium atoms within the interlayer gaps. That indicated in Fig. 2,*b* would account for the formula $C_{64}Na$.

The compound with the suggested formula $C_{64}Na$ is the only sodium-graphite compound which has been detected in our work in spite of the range of experimental conditions used. Thus sodium apparently differs from the heavier alkali metals; potassium, for example, is reported[2] to form a range of compounds between C_8K and $C_{60}K$.

Fuller reports, including the results of the more detailed structural analysis and also measurements of the physical effects of sodium on graphite, will be published elsewhere.

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Ferric Perchlorate Complex Ion

Sutton[1], and later Sykes[2], have reported association constants for the formation of ferric perchlorate complex ion, $FeClO_4^{++}$, although other investigators have been unable to find evidence for the existence of this species[3,4]. Inasmuch as perchloric acid and perchlorate salts are commonly used for maintaining constant ionic strength in physical-chemical measurements on the assumption that perchlorate ion does not complex appreciably with cations in aqueous solution, the question of the existence and extent of perchlorate complexing is a matter of some importance.

Both Sutton[1] and Myers[5] have suggested that perchlorate complexing might play a part in the kinetics of the iron(II)–iron(III) electron-exchange reaction. For reaction paths involving hydrated but otherwise uncomplexed ferrous ion and ferric species the rate of this exchange reaction, generalizing the results of Silverman and Dodson[6], may be represented by

$$R = \sum_X \sum_{n=0}^{n=6} k_n (FeX_n^{3-mn}) (Fe^{++}) \qquad (1)$$

where R is the rate of exchange, X is any complexing ligand (including hydroxide ion) of charge m, and k_n is the specific reaction-rate constant for the exchange process in which Fe^{++} and FeX_n^{3-mn} are reactants. Silverman and Dodson[6] and Hudis and Wahl[7] have assumed that k_0, the specific reaction-rate constant for the exchange process involving uncomplexed ferric ion, has a value other than zero, that is to say, that exchange occurs between ferrous and uncomplexed ferric ion. Experiments which substitute heavy water for water as solvent[8], however, indicate that the situation is more complicated than initially supposed.

Alternatively, if k_0 is nearly zero, and if $FeClO_4^{++}$ is the only ferric perchlorate complex formed, assuming a value for k_1, the specific reaction-rate constant for the exchange process involving $FeClO_4^{++}$ on the basis of the values for other monovalent complexing anions, the value of the formation constant, K_1, for the ferric monoperchlorate complex ion can be estimated from available kinetic data on the exchange reaction since,

$$(FeX_n^{3-mn}) = K_n (Fe^{+++}) (X)^n \qquad (2)$$

and the rate contributions of hydrolysed ferric species are known. According to the data of Silverman and Dodson[6], at 21·6° C. and an ionic strength of 0·55:

$$k_1 K_1 = k/(ClO_4^-) = 3·3/0·55 = 6·0 \text{ sec.}^{-1} \text{ f.}^{-2} \qquad (3)$$

where k is a specific reaction-rate constant determined by Silverman and Dodson and equated by them to k_0. Except for $FeOH^{++}$, the reactions of ferrous ion with dipositive ferric complexes have specific rate constants of comparable magnitude and apparently related to the size of the complexing anion. If the value of k_1 is estimated to be between 1 and 10 sec.$^{-1}$ f.$^{-1}$ by analogy with the values of 14, 20, 29 and 40 sec.$^{-1}$ f.$^{-1}$ for bromide[9], thiocyanate[9], chloride[6] and fluoride[7] ions respectively, then the resulting formation constant for the ferric monoperchlorate complex ion is between 6 and 0·6 f.$^{-1}$. The values reported by Sykes[2] on the basis of his own studies and calculated by him from data of others and from Bjerrum's theory, corrected to an ionic strength of 0·55 using the semi-empirical equation[3] and ignoring small temperature differences, namely, 2·8, 2·0 and 4·4 f.$^{-1}$ respectively, all lie well within these limits.

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Differential Thermal Method for determining Reactivity of Calcium Oxide

It has been reported[1] that the extent to which the reaction

$$CaO + MgCO_3 \rightarrow CaCO_3 + MgO$$

proceeds is influenced by the reactivity of the calcium oxide. The possibility that both this and the analogous reaction

$$CaO + Mg(OH)_2 \rightarrow Ca(OH)_2 + MgO$$

might be used for determining the reactivity of calcium oxide was systematically investigated, using differential thermal analysis. A hydration calorimetric technique and determinations of specific surface area by nitrogen adsorption were used for comparative purposes.

On the basis of the established fact that the reactivity of calcium oxide decreases as the temperature of calcination increases[2], samples of calcium oxide of widely differing reactivity were prepared by heating reagent quality calcium hydroxide at 100 deg. C. temperature intervals over the range 500–1,400° C. and precipitated calcium carbonate over the range 900–1,400° C. Thermograms for mixes of equimolecular quantities of such calcium oxides and magnesite are presented in Fig. 1 as curves (*a*) to (*e*) inclusive. Only one curve, (*e*), is given for the oxide from the carbonate because the curves were virtually identical for all such oxides prepared. The thermograms for the mixtures containing calcium oxide derived from calcium hydroxide at temperatures up to 1,100° C. show an exothermic peak at about 380° C., due to the net exothermal effect of the reaction. The subsequent endotherms represent respectively the decomposition of the remaining magnesite and the calcium carbonate formed as a result of the interchange reaction. The fact that, for the comparatively inactive forms of oxide where no interchange exotherm was found, considerable amounts of calcium carbonate were nevertheless formed, shows that its formation also proceeds as a result of reaction between the calcium oxide and the carbon dioxide formed during the magnesite decomposition over the range 600–700° C.

Three trends can be seen from these results. First, as the reactivity of the calcium oxide decreases, so the extent of the exothermal interchange reaction decreases until, for the oxide obtained from calcium

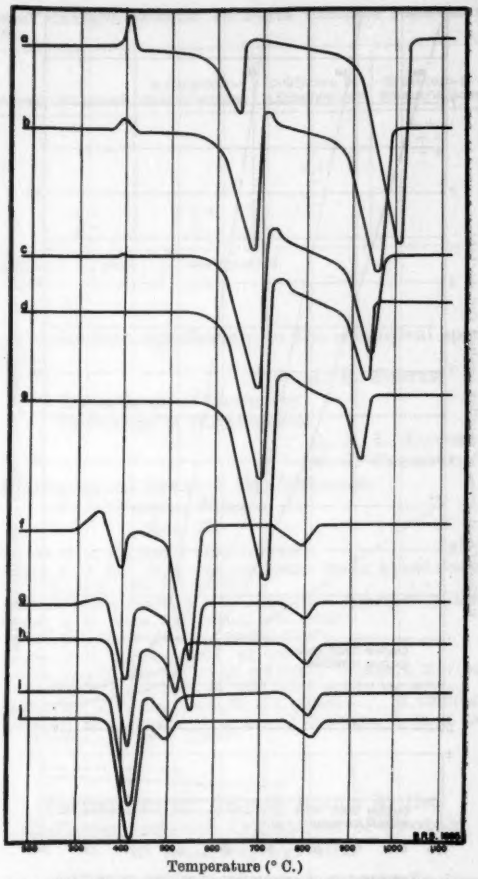

Fig. 1. Thermograms for equimolecular mixtures of magnesite or magnesium hydroxide and calcium oxide of origin indicated: (*a*)–(*d*), Magnesite and CaO derived from $Ca(OH)_2$ at (*a*) 500° C., (*b*) 900° C., (*c*) 1,100° C., (*d*) 1,400° C.; (*e*), magnesite + CaO derived from $CaCO_3$ at 900° C.; (*f*)–(*i*), magnesium hydroxide + CaO derived from $Ca(OH)_2$ at (*f*) 500° C., (*g*) 900° C., (*h*) 1,100° C., (*i*) 1,400° C.; (*j*), magnesium hydroxide + CaO derived from $CaCO_3$ at 900° C.

hydroxide at 1,200° C. or higher, and for all oxide obtained from calcium carbonate, it is absent. Secondly, the area of the magnesite peak increases as the reactivity of the oxide decreases, and thirdly, the calcium carbonate peak area shows a corresponding decrease. The relationship between the oxide reactivity as reflected by the ratio of thermoactive reaction product and reactant peak areas and its temperature of preparation is presented in Fig. 2 in curves (*a*) and (a_1) respectively for the oxide obtained from the hydroxide and the carbonate.

Thermograms for the magnesium hydroxide–calcium oxide mixes are shown in Fig. 1 as curves (*f*)–(*j*) inclusive. For the most reactive oxides the exothermal peak above 300° C. is evident; as the oxide reactivity and the extent of reaction decreases, the subsequent magnesium hydroxide peak increases and the calcium hydroxide reaction product peak decreases. From the peak area ratios illustrated in Fig. 2, curves (*a*), (a_1), (*b*) and (b_1) the parallel trends of the results for the two reactions investigated are evident; the results of the specific surface area determinations (curves (*c*) and (c_1)) and the hydration

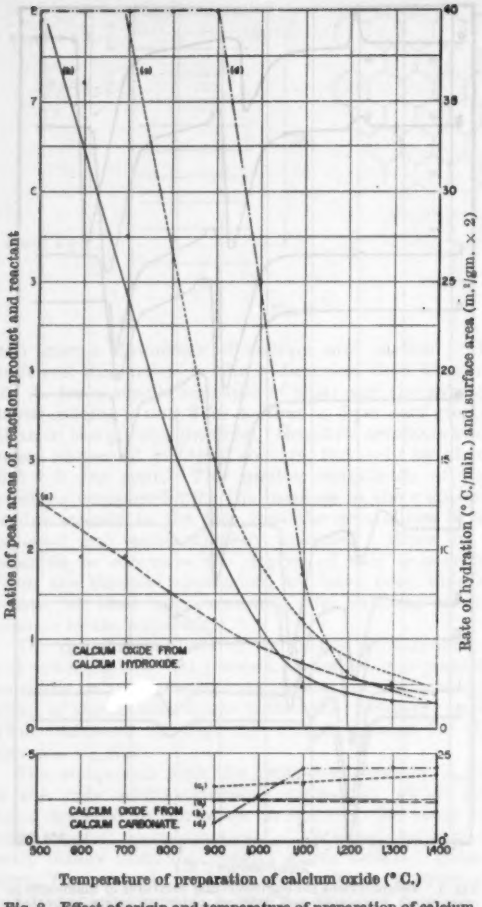
Ratios of peak areas of reaction product and reactant
Rate of hydration (° C./min.) and surface area (m.²/gm. × 2)
CALCIUM OXIDE FROM CALCIUM HYDROXIDE
CALCIUM OXIDE FROM CALCIUM CARBONATE
Temperature of preparation of calcium oxide (° C.)

Fig. 2. Effect of origin and temperature of preparation of calcium oxide on ratio of peak areas of reaction product and reactant for magnesite–calcium oxide reaction (— — —), and magnesium hydroxide–calcium oxide reaction (———); on surface area (– – – – –) and on rate of hydration (—·—·—·—) of the calcium oxide

calorimetric data (curves (d) and (d_1)) demonstrate that these latter-established techniques yielded somewhat similar results. The decrease in the reactivity of the oxide obtained from calcium hydroxide with increasing calcination temperature and the virtual independence of reactivity on calcination temperature for that prepared from precipitated calcium carbonate emerge clearly. However, by these techniques of differential thermal analysis, the reactivity of calcium oxide prepared from calcite was found to decrease with increasing calcination temperature, as reported by Murray, Fischer and Sabean[3] and others.

While the hydration calorimetric technique shows marked differences in the reactivity of the oxide from calcium hydroxide calcined over the range 900–1,400° C., it does not, under the experimental conditions used, discriminate between oxides of widely differing reactivity prepared over the range 500–900° C.; differential thermal analysis, for both reactions, shows marked differences of reactivity for oxide prepared in the range 700–1,100° C. for the exothermic reactions and over the wider range 500–1,400° C. for the reaction product–reactant peak area ratio.

Slight hydration of the calcium oxide, corresponding to an ignition loss of about 5 per cent, profoundly reduced its reactivity as evaluated by the hydration calorimetric technique. In contrast, with the technique of differential thermal analysis, hydration of calcium oxide does not reduce the measured reactivity.

A detailed account of this work will be published elsewhere. This communication is published by permission of the South African Council for Scientific and Industrial Research.

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Action of Periodate on some Polysaccharides containing Aldoheptose Sugars

Information about the molecular localities of the heptose residues in certain polysaccharides[1] may be obtained by application, in sequence, of periodate oxidation, reduction with sodium borohydride and acidic hydrolysis.

The following experimental procedure was adopted: aqueous solutions of polysaccharide (1 vol., 1 per cent w/v) and sodium metaperiodate (0·5 vol., 2 per cent w/v) were mixed at room temperature (*c.* 20°). At suitable time-intervals, aliquots (2 ml.) were withdrawn and treated immediately with ethylene glycol (0·5 ml.). After standing for 15 min. a fresh aqueous solution of sodium borohydride (2 ml., 1 per cent w/v) was added. After a further 15-min. period glacial acetic acid (1 ml.) was added to each aliquot, which was diluted to 8 ml., dialysed overnight against running tap-water and freeze dried. Samples of these products were used for aldoheptose and hexose determination by the Dische methods[2], further samples of 3–10 mgm. were hydrolysed in ampoules with 2 *N* hydrochloric acid (0·5 ml.) at 100° for 3 hr., the hydrolysates diluted to 5 ml. and neutralized by di-*n*-octylmethylamine[3]. The hydrolysates were freed from amino-sugars by 'Amberlite *IR*-120' (H+), neutralized[3], concentrated and analysed by zone electrophoresis (enclosed strip method[4], borate buffer *p*H 10) and paper chromatography[5]. The results are summarized in Fig. 1 and Table 1.

The heptose content of the *Chromobacterium violaceum* polysaccharides was rapidly reduced by 90 per cent or more whereas about 30 per cent of that of *Pasteurella pestis* polysaccharide remained intact (Fig. 1) and could be detected by paper chromatographic and zone electrophoretic analysis (Table 1). There was a corresponding increase in the hexose content in the initial stage of the reaction.

Thus in the polysaccharides studied it appears that there may be at least two modes of linkage of the heptose units, in one of which the heptose is very rapidly attacked by periodate.

Table 1. CHANGE IN NITROGEN-FREE SUGAR CONTENT OF SOME BACTERIAL POLYSACCHARIDES ON PERIODATE OXIDATION FOLLOWED BY REDUCTION

Polysaccharide source	Time of periodate oxidation (min.)	Glucose	Galactose	Mannose	Rhamnose	D-Glycero-D-galactoheptose	D-Glycero-D-mannoheptose	L-Glycero-D-mannoheptose
*Pasteurella pestis** (ref. 6)	0	++		−				+++
	1–2	++		++				++
	4–8	++		++				++
Chromobacterium violaceum N.C.T.C. 7917 (ref. 7)†	0	+	+++	−			++	
	2	+	+++	++			+	
	4–8	+	+++	++			−	
C. violaceum (BN)* (ref. 8)	0		−		++	+++		
	8		+++		++	−		

+++, Major component; ++, moderate amount; +, trace; −, not detected.
* Also contains *N*-acetyl-D-glucosamine.
† Also contains *N*-acetyl-D-fucosamine.

In the reaction sequence adopted the heptose was converted to a hexose which, in each instance, was different from that originally present in the polysaccharide (Table 1) and, moreover, was that anticipated from the structures allocated (only tentatively for *P. pestis* and *C. violaceum* 7917) to the parent heptoses.

A possible structural arrangement which would account for the above observations would involve the presence of heptopyranose units with unsubstituted $C_{(3)}$- and $C_{(7)}$-hydroxyl groups, as terminal or internal units in the polysaccharide chains.

Conversion of the heptose units in the polysaccharides to hexose units may offer a means of simplifying structural determination of the polymers.

An attempt to relate the structural changes induced to loss of serological specificity could only be attempted with the *C. violaceum* (7917) polysaccharide. Serological activity was tested by inhibition of hæmagglutination using red cells sensitized with the parent lipopolysaccharide[9]. In this instance all serological activity appeared to have been lost after exposure to periodate for 30 sec.; reduction with sodium borohydride alone, however, caused no detectable loss of serological activity. This agrees with the widely held view that the reducing end residue does not contribute significantly to the serological specificity.

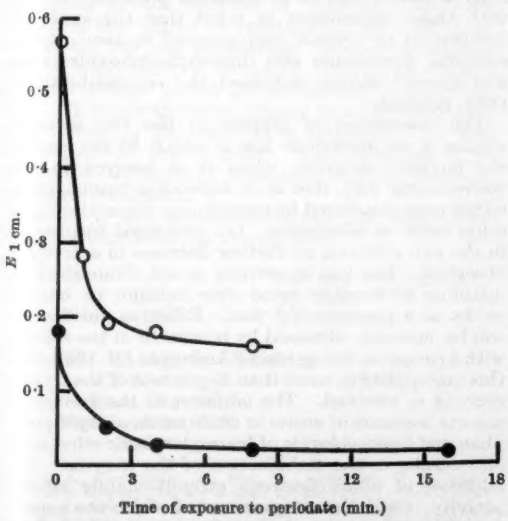

Fig. 1. Loss of aldoheptose sugars from bacterial polysaccharides on periodate oxidation. Heptose estimated colorimetrically (ref. 2) and expressed as extinction 505–540 mμ. ○, *P. pestis* polysaccharide; ●, *C. violaceum* (7917) polysaccharide; the identity of the heptose sugars has not been rigorously established, but they are believed to be L-glycero-D-mannoheptose and D-glycero-D-mannoheptose respectively

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Formation of >C=O Bonds in the Radiation-induced Oxidation of Protein in Aqueous Systems

A SERIES of observations recently made indicate that formation of the >C=O bond is a principal chemical effect of the attack of hydroxyl radicals on protein in oxygenated solution. Irradiations were made in a neutron flux produced by bombardment of beryllium with 24-MeV. deuterons in the Crocker Laboratory 60-in. cyclotron. Solutions were exposed under one atmosphere of oxygen in sealed 'Pyrex' cells mounted in a motor-driven reel situated 15 cm. from the beryllium target. One of the cells in each exposure contained a formic acid–oxygen dosimeter[1]. (We employed the formic acid–oxygen dosimeter to obtain an estimate of the relative number of water molecules decomposed via (1) $H_2O = H + OH$ and (2) $H_2O = \frac{1}{2}H_2 + \frac{1}{2}H_2O_2$. Absolute yield values could then be calculated in terms of G(molecules/100 eV.), since in dilute solutions at pH values above 2·5, the sum $G(1) + G(2) = G(H_2O)$ is to a first approximation independent of the quality of the absorbed radiation[1]. With our standard geometry for neutron exposures, we obtained $G(1) = 2{\cdot}68$, $G(2) = 0{\cdot}97$ on the basis of $G(H_2O) = 3{\cdot}65$[2].)

The following procedures were found applicable to the study of carbonyl formation in aqueous pepsin (twice crystallized, Lot No. 9477 Nutritional Biochemicals Corporation, Cleveland, Ohio) solutions (6·0 mgm./ml., 0·5 M in sodium sulphate, 10^{-3} M in sulphuric acid)[3]. Immediately after irradiation, platinum black was added to destroy hydrogen peroxide, and the solution was heated to obtain the

two fractions, precipitate (I) and supernatant (II). Each was treated with 2,4-dinitrophenylhydrazine–hydrochloric acid solution[4] and dialysed to remove excess reagent and any products of low molecular weight. An appropriate aliquot of the dialysed protein was then added to methanol–potassium hydroxide solution for spectrophotometric analysis after the method of Lappin and Clark[5]. These authors have shown for monocarbonyl 2,4-dinitrophenylhydrazones that the position of the absorption maximum, as well as the value $E_{max.}$, is nearly independent of the structure of the carbonyl compound. We found that the absorption spectrum of fraction I ($E_{max.}$ = 480 mμ) corresponded to the spectrum of authentic monocarbonyl hydrazones and, by using the formaldehyde derivative as a standard, obtained the value[2] $G = 1{\cdot}2$ for monocarbonyl production at a dose equivalent to $2{\cdot}5 \times 10^{19}$ eV./ml. Fraction II showed a blue colour and absorption spectrum ($E_{max.}$ = 550 mμ) which can be attributed to 1,2-dicarbonylhydrazones. Using an authentic glyoxal derivative as a standard, we obtained $G = 0{\cdot}02$ for dicarbonyl production. The pepsin in unirradiated control solutions showed no retention of 2,4-dinitrophenylhydrazine. The position of carbonyl formation in the radiation-induced oxidation of pepsin is now under investigation; the relative importance of the various main and/or side-chain bonds is being determined.

Independent evidence for formation of carbonyl groups through processes induced by attack of hydroxyl radicals at the peptide linkage has been obtained in a parallel study of the oxidation of gelatin (purified pigskin, Lot No. 60–6657, Eastman Organic Chemicals solution) (40 mgm./ml. in air-saturated solution) by Fenton's reagent in which case hydroxyl radicals are formed by the reaction[6] $Fe^{2+} + H_2O_2 = Fe^{3+} + OH^- + OH$. We have found in this system (*a*) that the dialysed protein contains approximately one carbonyl group for every two hydroxyl radicals formed and (*b*) that glyoxylic, pyruvic, and α-ketoglutaric acids are among the carbonyl compounds liberated on hydrolysis of the dialysed fraction with 2 *N* hydrochloric acid solution under oxygen-free conditions. Identification of the keto-acids is based on the observation that the 2,4-dinitrophenylhydrazone derivatives could not be distinguished chromatographically[7] from the corresponding authentic material. (Formation of these keto acids is consistent with a previously suggested mechanism for the radiation-induced rupture of the peptide linkage[8]; $RCONHCR_2 \rightarrow RCON{=}CR_2 \xrightarrow{H_2O} RCONH_2 + R_2CO$. We have recently found that irradiated solid pepsin on dissolution in water also yields high-molecular-weight products containing carbonyl groups and suggest that imino intermediates are also involved in direct action: $RCONHCR_2 \rightarrow RCON{=}CR_2 + H_2$.)

The possible biological importance of carbonyl formation is indicated by the observation that the addition of 2,4-dinitrophenylhydrazine to irradiated (4×10^{17} eV./ml.) rat liver homogenate[9] (50 mgm./ml. in water) gave a non-dialysable hydrazone fraction that exhibited an intense magenta colour on mixing with methanol–potassium hydroxide. (We estimate that this technique, with slight modification, could be used for measuring an X-ray dose of a few hundred roentgens.) Control tests with unirradiated homogenate showed a negligible background colour. The literature[10] indicates that radiation-induced oxidation of a number of different types of chemical linkages may contribute to carbonyl production in a biological system.

A detailed report of this work will appear in a forthcoming paper.

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Acetylation of Trypsin in Organic Solvents

Previous work in this laboratory has shown that treatment of trypsin with various anhydrides results in the formation of the corresponding acyltrypsin derivatives, each possessing some interesting properties of its own, while all retained their original proteolytic activity[1]. From the derivatives so far prepared, acetyltrypsin is the best-characterized chemically modified enzyme[2], while succinyltrypsin is the most stable of the derivatives so far obtained[3]. The use of water as a solvent in investigating enzyme modification sets some definite limitations as to type of reagent usable and as to reactions possible. It was with these experiences in mind that the study of acetylation of trypsin was pursued in two organic solvents, formamide and dimethylsulphoxide, Rees and Singer[4] having indicated the compatibility of these systems.

The dissolution of trypsin in the two solvents results in an immediate loss of about 50 per cent of the enzymic activity, when it is assayed in the conventional way, that is, in aqueous solutions, using either urea-denatured hæmoglobin or benzoylarginine ethyl ester as substrates. On prolonged incubation in the two solvents, no further decrease in activity is observed. The loss in activity is not diminished by distilling formamide twice over calcium or barium oxide, at a pressure of 1 mm. Effective purification can be, however, obtained by treatment of the solvent with a cation exchange resin ('Amberlite *IR*-120'). By this manipulation, more than 85 per cent of the tryptic activity is retained. The addition to the solvent of minute amounts of acetic or citric acids, acetyltryptophan and hydrochloride of benzoylarginine ethyl ester also results in the stabilization of the enzyme. The addition of alkali destroys proportionately tryptic activity, while the temperature at which the experiments are carried out is without noticeable effect. The dissolution of trypsin in organic solvent–water mixtures with increasing water content does not stabilize the enzyme. On the contrary, there is a relatively narrow region containing about 55–65 per

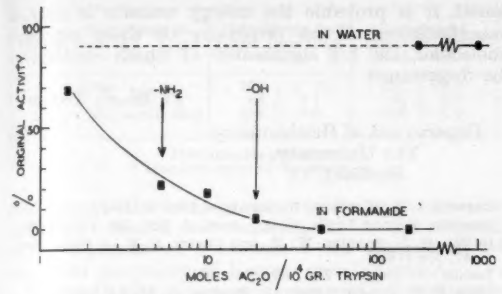

Fig. 1. Inactivation of trypsin by acetic anhydride in formamide. Trypsin concentration: 5 mgm./ml. of formamide; acetylation for 1 hr. at 22° C. Two stoichiometric points are indicated, corresponding to the total number of amino- and hydroxyl-groups present in trypsin

cent of organic solvent, in which the enzyme loses up to 80 per cent of its activity on dissolution in 'Amberlite'-untreated formamide. In treated formamide, or dimethylsulphoxide, this is not observed.

The acetylation of trypsin in the two solvents is accompanied by a loss of enzymic activity in proportion to the amount of anhydride employed. While in buffered aqueous solutions huge excesses of the anhydride are without detrimental effect, a total loss of activity is observed in formamide, at 22° C., when about 50 equivalents of anhydride are employed per mole of trypsin. This amount corresponds approximately to the total number of hydroxyl groups in trypsin. The correlation of activity and amount of anhydride is recorded in Fig. 1.

The extent of substitution of amino- and hydroxyl-groups in trypsin, acetylated with an amount of anhydride corresponding to the total hydroxyl groups of the enzyme, was found to be 95 per cent and 7·5 per cent respectively. This proportion is essentially identical with that observed in the course of acetylation of the enzyme in aqueous solutions[2]. Therefore, the degree of substitution by itself does not explain the loss in activity. At higher proportions of anhydride, a complete acetylation of the hydroxyl groups occurs in formamide, but not in water. It would therefore appear that the difference in specificity of the reaction is responsible for the inactivation of the enzyme. The inactivation is irreversible, and activity cannot be regained by mild hydrolytic treatment.

This study was carried out under the auspices of the U.S. Atomic Energy Commission.

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Carotenoid-sensitized Fluorescence of Chlorophyll *in vitro*

It is well established that light absorbed by carotenoids can contribute to photosynthesis, phototaxis and the red fluorescence shown by chlorophyll *A* in the intact chloroplast. In *Chlorella*, independent measurements of both fluorescence[1] and photosynthesis[2] indicate a 40–50 per cent efficiency of energy transfer from β-carotene and lutein to chlorophyll *A*. In brown algae[3,4] almost complete transfer of energy from fucoxanthol to chlorophyll *A* is reported.

Table 1

Carotenoid		Porphin	Approximate transfer efficiency (per cent)
β-Carotene	transfer to	Chlorophyll *A*	10
,,	,, ,,	Phæophytin *A*	0
Lutein	,, ,,	Chlorophyll *A*	60
,,	,, ,,	Chlorophyll *B*	10
,,	,, ,,	Phæophytin *A*	0
Fucoxanthol	,, ,,	Chlorophyll *A*	100
,,	,, ,,	Chlorophyll *B*	20
,,	,, ,,	Phæophytin	0

This transfer has now been observed in artificial systems, in which the concentration of pigments approaches that found in chloroplasts. Pure plant pigments, freshly prepared by chromatography, were dispersed in detergent micelles[5] to give local concentrations of about 0·1 *M*. In this way, equimolecular mixtures of carotenoid and chlorophyll or phæophytin were prepared. Electronic energy transfer was measured by comparing the fluorescence excitation spectra of chlorophyll or phæophytin in the presence and absence of carotenoid. The exciting wave-lengths ranged between 380 and 670 mμ. Two of the resulting spectra are shown in Fig. 1, together with the difference spectra representing the contribution of fucoxanthol and lutein respectively to chlorophyll *A* fluorescence. Table 1 shows approximate transfer efficiencies for several pairs of pigments all at roughly 0·1 *M* concentration.

These results show that for lutein and fucoxanthol, transfer to chlorophyll *A* in micelles is similar in

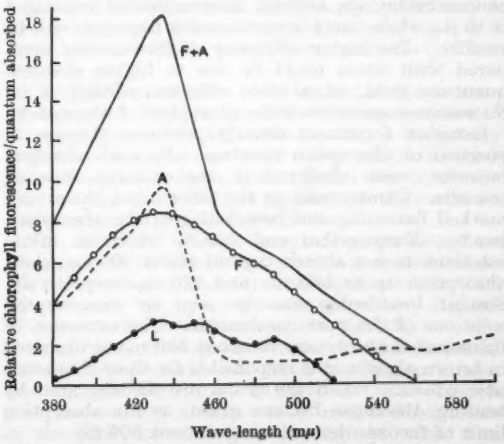

Fig. 1. Electronic energy transfer from carotenoids to chlorophyll *A* in detergent micelles. *A* and *F* + *A*, Fluorescence excitation spectra of chlorophyll *A* alone, and an equimolecular mixture of *F*, fucoxanthol and chlorophyll *A*; *F* and *L*, difference spectra measuring electronic energy transfer from fucoxanthol and lutein respectively to chlorophyll *A*. Ordinate: Relative numbers of quanta absorbed which appear as chlorophyll *A* fluorescence; abscissa, wave-length of exciting light in mμ

magnitude to that reported for chloroplasts. The value for β-carotene may be too low, as difficulty was encountered in dispersing this material in micelles.

The low value for carotenoid–chlorophyll *B* transfer, and zero transfer to phæophytin *A* are surprising, if only the intermolecular distance and estimated overlap of absorption and emission spectra are considered. These are similar for both the chlorophylls and phæophytin *A*. It is evident[6], however, that proximity and overlap are not the only factors necessary for transfer. The ratio of the fluorescence polarization measured with radiation absorbed mainly by fucoxanthol, to that excited by radiation absorbed only by the red band of chlorophyll *A*, was determined. In chlorophyll *A* alone this ratio is 23 per cent, while in the fucoxanthol–chlorophyll *A* system this value is increased to 70 per cent. This increase in positive polarization illustrates the preferred coupling of fluorescence and absorption oscillators which the transfer involves. High positive polarizations are only found in chlorophyll *A* beyond 600 mμ.[7], so that the fluorescence spectrum of fucoxanthol presumably extends into this region.

It is tempting, in view of the high efficiency of transfer to chlorophyll *A* compared with chlorophyll *B* and phæophytin *A*, to assume a specific complex to exist.

However, an approximate calculation indicates that the observed transfer values could exist in a random array of pigment molecules. By an absolute method[8], the carotenoids can be shown to have quantum yields not greater than 10^{-4}. If this value is taken as a maximum value, the fluorescence lifetime must be of the order of 10^{-12} sec. For a normal lifetime of 10^{-8} sec. we can assume a half-transfer radius of about 40 A., taking into account the overlap integral and the transfer radii reported for xanthydrol dye molecules[9]. Assuming transfer by the mechanism of coupled oscillators, d the half-transfer radius for a fluorescence lifetime of 10^{-12} sec. is given by

$$\left[\frac{40}{d}\right]^6 = \frac{10^{-8}}{10^{-12}} \text{ or } d = 8{\cdot}6 \text{ A.}$$

In an equimolar mixture of pigments at 0·1 *M* concentration the average intermolecular spacing d is 10 A., while many intermolecular distances will be smaller. The higher efficiency of fucoxanthol compared with lutein could be due to higher absolute quantum yield, or a more efficient overlap of its fluorescence spectrum with chlorophyll *A* absorption.

Complex formation usually produces changes in position of absorption maxima. No such changes, however, were observed in the systems showing transfer. Carotenoids, on the other hand, show very marked flattening and broadening of the absorption bands. Fucoxanthol and lutein, which in dilute solutions do not absorb beyond about 500 mμ, show absorption up to 560 mμ and 520 mμ respectively. Similar broadening can be seen in concentrated solutions of the pure carotenoids. The extension of fucoxanthol absorption to about 560 mμ is observed in brown algae and is responsible for their characteristic colour. When the grana are disintegrated by heating, the algae become green, as the absorption limit of fucoxanthol returns to about 500 mμ.

In brown algae, fucoxanthol is an efficient substitute for chlorophyll *B*, filling in the region 460–540 mμ where chlorophyll *A* is relatively transparent, and in these plants the fucoxanthol–chlorophyll *A* ratio approaches unity. In green plants however, where the fractional absorption of carotenoids is small, it is probable the energy transfer is only a manifestation of the proximity of these pigment molecules, the full significance of which remains to be determined.

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Effects of Orotic Acid on Coenzyme A and Pantothenic Acid of Liver in Vitamin B_{12} Deficiency

Orotic acid was found to reduce the vitamin B_{12} requirement for growth and for the betaine–homocysteine transmethylase activity of liver in rats fed corn/soybean meal ration deficient in vitamin B_{12}[1]. These results suggested a metabolic relationship between orotic acid and vitamin B_{12}. To ascertain whether this metabolic relationship was limited or more extended, we have directed our research towards other biochemical modifications reported in vitamin B_{12} deficiency.

Since an increase in concentration of liver pantothenic acid[2] and coenzyme A[3] was observed in animals deficient in vitamin B_{12}, we have extended our investigation to the effect of orotic acid on content of total pantothenic acid and coenzyme A of liver in rats deficient in vitamin B_{12}.

To this end, fifteen weanling male rats of the Sprague–Dawley strain, weighing 40–45 gm., were employed. The animals were divided into three groups, all of which were fed the basal corn/soybean meal ration deficient in vitamin B_{12} as proposed by Register *et al.*[4]. Iodinated casein or desiccated thyroid was omitted. Group 1 served as control and received only the basal ration. Group 2 was fed the basal ration supplemented with vitamin B_{12} (2γ/100 gm. of diet). Group 3 received the basal ration supplemented with orotic acid (1,000γ/100 gm. of diet). The animals were maintained on their respective ration for 8 weeks and weighed weekly. Feed and water were supplied *ad libitum*. At the end of the experiment the animals were killed by decapitation and bled. Total pantothenic acid and coenzyme A were evaluated in the liver of each animal. To liberate pantothenic acid from liver tissues, a weighed portion of each liver was homogenized in a Potter glass-homogenizer with four volumes of water. 2 ml. of this homogenate were placed in a 125 ml. Erlenmeyer flask with 40 ml. of water and 1 ml. of toluene. The materials were incubated for 16 hr. at 37° C. After incubation the samples were boiled, cooled, adjusted to 100 ml. and filtered. Total pantothenic acid was assayed, according to the method of Skeggs and Wright[5], in aliquots of the filtrate, utilizing *L. arabinosus* 17/5 *ATCC* 8014 as a test organism. For evaluating coenzyme A, a weighed portion of the same liver used for studies on pantothenic acid content, was homogenized with ten volumes of ice-

Table 1

Group No.*	Supplement to 100 gm. basal ration	Pantothenic acid (γ/gm. liver fresh weight)	Coenzyme A (units/gm. liver fresh weight)
1	None	377 ± 27·6†	207 ± 12·7
2	2γ vitamin B_{12}	86 ± 8·1	81 ± 3·3
3	1,000γ orotic acid	234 ± 16·2	148 ± 8·8

* Five animals per group.
† Standard error of the mean.

cold water. The homogenate was boiled in a water bath, then cooled and centrifuged. In the aliquots of the supernatant, coenzyme A was determined by the usual procedure of Kaplan and Lipmann[6].

The results for liver pantothenic acid and coenzyme A are presented in Table 1. A striking increase in content of pantothenic acid and of coenzyme A concentration in the liver of rats kept on the vitamin B_{12}-deficient diet (group 1) for 8 weeks, is evident. These values appear much lower in rats of group 2 fed vitamin B_{12}. These results confirm the results previously reported in the chick and in the rat[2,3]. Orotic acid fed to vitamin B_{12}-deficient rats (group 3) exerts a vitamin B_{12}-like action in lowering the content of pantothenic acid and coenzyme A in the liver of male rats. Nevertheless, the effects of orotic acid are less, under our experimental conditions, than those shown by vitamin B_{12} itself. Therefore, orotic acid is not to be considered only as a factor in promoting growth and stimulating liver betaine–homocysteine transmethylase activity in rats deficient in vitamin B_{12}[1], since it also participates in the regulation of pantothenic acid and coenzyme A in the liver of male rats. These findings may point to a more extended metabolic relationship between vitamin B_{12} and orotic acid.

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Enzymatic Formation of Aminomalonic Acid from Ketomalonic Acid

Recent work in this laboratory demonstrated the occurrence of a new enzyme, aminomalonic acid decarboxylase, in the posterior silk-gland of silkworm and in rat liver[1]. The fact that this enzyme was first observed in silkworm, which has a remarkably high activity of glycine synthesis[2], seems to suggest a physiological significance of the enzyme in the biosynthesis of glycine, although natural occurrence of this amino-acid has hitherto not been reported.

In the present communication, we report the formation of aminomalonic acid by an alanine-ketomalonate (mesoxalate) transaminase from silkworm and animal tissues. The first observation of this enzyme activity was reported by Braunstein in 1939 with pigeon-breast muscle; however, no attempt was made to identify the reaction product, aminomalonic acid, nor to characterize the enzyme further[3]. Later, Green *et al.* found that the purified glutamate–aspartate transaminase of pig heart showed a slight activity toward ketomalonate in place of oxalacetate[4].

The enzyme solution was prepared by homogenizing the posterior silk-glands of silkworms at the fifth instar stage with 3 vol. of 0·15 *M* potassium chloride solution (*p*H 7·8) and centrifuging off the undissolved material. The incubation mixture contained 1·0 ml. of the enzyme solution (approximately 1·0 mgm. N), 30 μmol. of disodium ketomalonate, 60 μmol. of DL-alanine, and 200 μmol. of borate buffer (*p*H 9·0) in a total volume of 3·0 ml. The mixture was incubated for 40 min. at 37° and deproteinized with sulphuric acid and sodium tungstate.

For the determination of the aminomalonate formed a simple method was devised: an aliquot taken from the supernatant solution was diluted with hydrochloric acid to give a final concentration of 0·1 *N* and heated for 30 min. in a boiling water-bath. This treatment proved to be sufficient for converting aminomalonic acid to glycine quantitatively. The glycine thus formed was determined by the method of Krueger[5]. Aminomalonic acid was also identified by paper chromatography and paper ionophoresis. The keto-acids were identified as their 2,4-dinitrophenylhydrazones by the method of Cavallini[6].

The activity of this enzyme solution (on the seventh day of the fifth instar stage), expressed as μmol. of the aminomalonate formed under the above condition, was about 9·2.

The *p*H optimum for the transamination of alanine–ketomalonate was found to be at 9·0–9·2. This is markedly different from those for the reactions of alanine–glutamate or alanine–aspartate, which have been reported to be 7·5 or 7·4, respectively[7]. This fact is interesting and may suggest that there might be a separate transaminase for this reaction from the alanine–glutamate or the alanine–aspartate enzymes. Transaminases with alkaline *p*H optima have been recently reported, namely, alanine–glyoxylate enzyme in silkworm (*p*H 9·0–9·5)[2], in *Pseudomonas* (*p*H 8·0–9·0)[8], and ornithine-α-ketoglutarate enzyme (*p*H 8·2–9·0)[9].

Of 17 different amino-acids studied in the presence of ketomalonate, alanine was the most active in forming aminomalonate. Glutamate, α-aminobutyrate, and aspartate also could act as amino-donors in decreasing order of reactivity. In the case of 13 other amino-acids there was no appreciable formation of aminomalonate from ketomalonate.

The activity of this enzyme preparation was 99 per cent inhibited by *p*-chloromercuribenzoate (10^{-3} *M*), 84 per cent by benzoquinone (10^{-3} *M*), 58 per cent by hydroxylamine (2×10^{-3} *M*), and 38 per cent by potassium cyanide (10^{-3} *M*).

Of all tissue homogenates tested for this enzymatic activity, the posterior silk-gland of silkworm showed the highest activity. Besides, it was observed that the activity of this tissue increased with time in the fifth instar stage. It may be noted that this tissue also exhibited the highest activity of aminomalonic acid decarboxylase; thus the distributions of these two enzymes in this tissue seem to be associated with each other. These facts may suggest a possible role of this transaminase in the biosynthesis

of glycine, participating in the following metabolic pathways:

(tartronate $\xrightarrow{10}$) ketomalonate $\xrightarrow{transaminase}$ aminomalonate $\xrightarrow{decarboxylase}$ glycine

The transaminase activity was also demonstrated in the homogenates of rat liver, rabbit liver and rabbit heart. The highest activity was observed with the rat-liver enzyme.

The enzyme was partially purified from the rat liver homogenate by fractionating with ammonium sulphate. The preparation was about 18 times as active as the starting material, but pyridoxal phosphate had no effect on the activity at this stage of purification.

Full details of this work will be reported in a future publication. Our thanks are due to Miss Aiko Kikuchi for her skilful technical assistance in this work.

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Insulin - Cortisone Relationship in Experimental Teratogenesis

It is well known that the administration of large amounts of cortisone to experimental animals is associated with the production of hyperglycæmia[1]. There is also evidence from clinical cases that in diabetes the therapeutic use of cortisone increases considerably the daily amount of insulin required to maintain the blood-sugar within normal limits[2]. The nature of this contra-insulin effect of cortisone is uncertain, but it has been suggested that it may be due to excessive destruction of insulin since there is no definite evidence that contra-insulin factors are responsible for a marked increase in the peripheral requirements for insulin[3].

In a series of experiments reported recently we used the teratogenic effects of hypervitaminosis A as a base line for the study of the action of cortisone during pregnancy[4,5]. In these experiments one group of pregnant female rats of the Wistar strain received 60,000 I.U. vitamin A acetate daily by gastric intubation from the eighth to thirteenth days of pregnancy. Another group received the same treatment with, in addition, 20 mgm. of cortisone acetate daily by subcutaneous injection from the ninth to twelfth days. A third group received only the cortisone treatment. It was found that in the young obtained from these rats on the twentieth day of gestation there was an incidence of deformities of the brain and calvaria of 7·8 per cent when the rats had received vitamin A alone during pregnancy, and of 36·6 per cent when they had received both vitamin A and cortisone. There were no deformities in the young from those mothers which received cortisone alone (Table 1). These results were interpreted as suggesting that the cortisone potentiated the teratogenic effects of the hypervitaminosis A, and it appeared that this potentiating action might in some way be related to a disturbance of carbohydrate metabolism.

Table 1

Treatment	No. of dams	Total No. of young	No. of young with brain deformities	Percentage of young with brain deformities
Vitamin A	12	77	6	7·8
Vitamin A, cortisone	12	41	15	36·6
Cortisone	12	73	0	0

We decided therefore, in view of the known contra-insulin effects of cortisone, to carry out further experiments in order to examine the possibility that insulin might exert a contra-cortisone effect in relation to the production of malformations. In these experiments one group of pregnant female rats was given vitamin A for the same period and in the same doses as in the earlier experiments. In addition, they received 1·5 units of protamine zinc insulin by subcutaneous injection, daily from the ninth to twelfth days of pregnancy. Another group received both vitamin A and cortisone in the same quantities as previously administered, but were also given 1·5 units of protamine zinc insulin daily by subcutaneous injection from the ninth to twelfth days. The results of these experiments are shown in Table 2. It will be seen that the incidence of deformities in the young fell from 36·6 to 1·2 per cent when insulin was given in addition to vitamin A and cortisone; and that while there was an incidence of 7·8 per cent in the young of dams which received vitamin A alone, no young with brain deformities were obtained from the dams which received insulin in addition to vitamin A.

Table 2

Treatment	No. of dams	Total No. of young	No. of young with brain deformities	Percentag young with brain deformities
Vitamin A, insulin	12	47	0	0
Vitamin A, cortisone, insulin	12	79	1	1·2

These findings indicate that the potentiating action of cortisone in relation to the production of malformations by hypervitaminosis A is wholly prevented by the administration of insulin. Further, it appears possible that insulin may have some effect in protecting the young against the teratogenic effects of hypervitaminosis A.

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Reversibility of the Inactivation of Rous Sarcoma Extracts by Detergents

THE infectivity of an aqueous extract from a Rous sarcoma is lost if cetyl trimethyl ammonium bromide ('Cetavlon'), a cationic detergent, is added to the active extract[1]. By further addition of an acid colloid substance, like ribonucleic acid[2] or heparin, to the non-infective system infectivity is restored.

From the results obtained by Pirie and co-workers[3–6] on some plant viruses, including the tobacco mosaic virus, treated with anionic detergents, and by Pfankuch and Kausche[7] on tobacco mosaic virus with cationic detergents, the mechanisms of the actions of the two types of detergents on tobacco mosaic virus appear to be different. We have explored the possibility of an analogous behaviour of Rous sarcoma extract towards the two types of detergent, with particular reference to the reversibility of inactivation. The results are given in Tables 1 and 2.

The extract was prepared with a Miller and Golder's buffer[8] 0·14 *M*, *p*H 7·0. The initial concentration of extract was 5 per cent. After centrifuging at 1,500 *g* for 60 min., hyaluronidase, 5 U.V./ml. (Ialovis Vister) was added as solubilizer, and after 30 min. the extract was centrifuged at 15,000 *g*. All operations were carried out at 3° C.

In the experiments with 'Cetavlon' the proportion detergent/infective extract, was 2 mgm./ml., and the amount of acid colloid (heparin) needed to restore infectivity was equal by weight to the amount of 'Cetavlon'. In the experiments with the anionic detergent (sodium lauryl sulphate), the proportions were 0·4 mgm./ml., and the amount of basic colloid (protamine) added was equal to the weight of anionic detergent. Inoculations (standard amount, 2 ml.) were made in the breast muscles of 8-week-old chickens, all from the same breed. They were killed between the thirteenth and fifteenth day of the experiment, at which time the first control animals were beginning to die.

It appears from the results obtained that, whereas the cationic detergent has a reversible action, as is shown by the restoration of infectivity in the inactivated system (extract + 'Cetavlon'), such a reversibility does not occur with the anionic detergent, even after addition of a basic colloid, like protamine.

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Effect of Creatine Phosphate Administration on the Cardiac Adenosine Triphosphate of Thyroxinized Rats

THE amounts of adenosine triphosphate, creatine phosphate and inorganic phosphorus in the heart of hyperthyroid animals are below normal. These amounts are restored to about the normal level by administration of adenosine triphosphate, whereas with cocarboxylase and organic phosphates restoration is incomplete[1]. Moreover, thyroxin intoxication reduces the creatine content of the skeletal muscle and the rate of creatine synthesis by liver[2].

We have sought to establish if creatine phosphate, which is essential in oxidative phosphorylation, affects the reduction of this process in the heart, induced *in vivo* by thyroxin in doses of 0·01 mgm. per animal.

0·01 mgm. D,L-thyroxin was daily injected intramuscularly into each of twenty-four rats, which were then divided into four lots. Lot *B* received no further treatment; lot *C* daily received 0·1 mgm. of buffered creatine phosphate per animal injected intramuscularly; lot *D* received daily 5 mgm. of buffered creatine per animal, injected intramuscularly; the rats in lot *E* each received a single injection of both creatine phosphate and creatine at the same doses as above. A fifth lot (*A*) of normal animals was used as control. After fifteen days the animals

Table 1. INACTIVATION AND REACTIVATION OF EXTRACTS FROM ROUS SARCOMA TREATED WITH CATIONIC DETERGENT ('CETAVLON')

Extract + heparin				Extract + 'Cetavlon'				Extract + 'Cetavlon' + heparin			
Inoculations in chickens	Developed tumours	Positive inoculations (per cent)	Mean weight developed tumours (gm.)	Inoculations in chickens	Developed tumours	Positive inoculations (per cent)	Mean weight developed tumours (gm.)	Inoculations in chickens	Developed tumours	Positive inoculations (per cent)	Mean weight developed tumours (gm.)
28	25	89	28	57	20	35	5	30	26	86	16

In the control chickens the ratio, developed tumours/inoculations was 70/76, with a positive inoculation of 92 per cent.

Table 2. INACTIVATION AND REACTIVATION OF EXTRACTS FROM ROUS SARCOMA TREATED WITH ANIONIC DETERGENT (SODIUM LAURYL SULPHATE)

Extract + heparin				Extract + sodium lauryl sulphate				Extract + sodium lauryl sulphate + protamine			
Inoculations in chickens	Developed tumours	Positive inoculations (per cent)	Mean weight developed tumours (gm.)	Inoculations in chickens	Developed tumours	Positive inoculations (per cent)	Mean weight developed tumours (gm.)	Inoculations in chickens	Developed tumours	Positive inoculations (per cent)	Mean weight developed tumours (gm.)
14	12	85	24	28	7	25	2	25	3	12	0·500

In the control chickens the ratio, developed tumours/inoculations was 70/76, with a positive inoculation of 92 per cent.

Table 1. Adenosine Triphosphate, Creatine Phosphate, Inorganic Phosphorus Creatine and Glycogen Content of Rat Heart Tissue after Daily Administration of Thyroxin and other Substances

Lot	Adenosine triphosphate (μgm./gm. fresh	Creatine phosphate (μgm./gm. fresh	Inorganic phosphorus tissue)	Creatine (mgm./100 gm. fresh tissue)	Glycogen
A (6)	331 ± 27	308 ± 23	521 ± 46	408 ± 16	183 ± 11
B (6)	126 ± 11	< 10	316 ± 34	293 ± 33	149 ± 18
C (6)	224 ± 30	167 ± 19	409 ± 43	307 ± 26	166 ± 22
D (6)	121 ± 16	< 10	325 ± 29	367 ± 38	151 ± 17
E (6)	230 ± 28	165 ± 21	418 ± 46	376 ± 41	164 ± 14

Values ± standard error

were killed by bleeding and determinations of adenosine triphosphate, creatine phosphate, inorganic phosphorus[3], creatine[4] and glycogen[5] were carried out.

As shown in Table 1, administration of creatine phosphate partially prevents the fall of adenosine triphosphate and creatine phosphate content of the heart in animals given thyroxin at high dosage. Creatine alone cannot do this, and not only the adenosine triphosphate content but also creatine phosphate content remain low.

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Phospholipids, Proteins and Platelet-Lipoid

The time taken for Russell viper venom to accelerate the clotting of fasting human plasma poor in platelets—the Stypven time—is relatively long. The addition of very small quantities of some active phospholipids[1,2] to this system produces a shortening of the Stypven time, and the shortening is proportional to the quantity added. It may be concluded that there is very little available active phospholipid in fasting platelet-poor plasma. A simple phospholipid extraction of fasting platelet-poor plasma yields a material that greatly accelerates the Stypven time of platelet-poor plasma. It is concluded that there are molecules of active phospholipids in plasma, but that they are 'unavailable' for this test and presumably are bound in the plasma proteins.

If a critical strength of an isolated phospholipid, active in the Stypven system, is added to platelet-poor plasma at 37° C. or 10° C. it is found that this activity has decreased at the end of 2 hr. (To 0·5 ml. of fasting platelet-poor plasma is added 0·5 ml. of a predetermined concentration of an active phospholipid in distilled water. 0·1 ml. of this mixture is transferred to another 0·1 ml. sample of stock plasma and the Stypven time determined. This should be about 14 sec. if the plasma alone has a Stypven time of 20 sec. At the end of 2 hr. a further 0·1 ml. of the mixture is transferred to 0·1 ml. of stock plasma and the Stypven time recorded. Another 0·5 ml. of plasma and 0·5 ml. of phospholipid are then mixed and 0·1 ml. transferred to 0·1 ml. of stock plasma and clotted as before to check that the plasma, Stypven and original phospholipid activity have not altered.) It may be concluded that less phospholipid is available after 2 hr. Plasma rich in heparin-clearing factor gives the same results as normal plasma. It is unlikely that any enzyme system has altered the chemical composition of the phospholipid since this reaction proceeds equally well at 10° C. as at 37° C. A possible explanation is that the phospholipid has become associated with the plasma proteins and is no longer 'available' for this test.

Intact platelets are inactive in accelerating the Stypven clotting time of plasma. If they are mechanically fragmented, a potent accelerator becomes available. The properties of fragmented platelets in all coagulation tests (except clot retraction) are identical with various cephalins including phosphatidyl-ethanolamine, phosphatidyl serine and phosphatidyl inositide[3] and it has been suggested that the activity of platelet-lipoid is due to its content of any one or a mixture of these or similar 'active' phospholipids.

If platelet fragments are incubated in a critical strength with platelet-poor plasma the acceleration of the Stypven time caused by their addition does not decrease in two hours. A protein-free phospholipid extract of platelets behaves like an active phospholipid extracted from any other source.

If the two previous conclusions are correct, this last observation may suggest that if platelet-lipoid is active in blood coagulation by virtue of a phospholipid prosthetic group, then there is another part of the molecule which prevents the whole molecule from becoming associated with the plasma protein and thereby losing its activity. In a natural state, platelet-lipoid is almost certainly associated with protein. The activity of platelets may, therefore, depend on a preferred orientation in the platelet-protein molecule of a prosthetic phospholipid group which is 'available', while the phospholipids in plasma proteins are differently orientated and inactive or 'unavailable'.

Further work is in progress to expand and confirm these tentative conclusions.

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Adrenocorticotrophic Hormone in the Blood of Adrenalectomized Rats

It is well known that stress results in increased activity of the adrenal cortex which is caused by an increased secretion of adrenocorticotrophin by the pituitary gland. However, the exact mechanism which controls the secretion of this hormone is not fully understood. Sayers and Sayers[1] suggested that stress causes an increased utilization of adrenocortical steroids by the peripheral tissues and the resulting low blood level of corticoids acts as a stimulus to the pituitary gland to increase its secretion

of adrenocorticotrophin. The Sayers hypothesis received convincing support from the results of the investigations of Taylor, Albert and Sprague[2], Gemzell, Van Dyke, Tobias and Evans[3] and Sydnor and Sayers[4], who found that animals with adrenocortical insufficiency exhibited a high blood level of adrenocorticotrophin. On the other hand, Barrett and Hodges[5] were unable to demonstrate any difference in plasma concentrations of adrenocorticotrophin between normal and adrenalectomized rats three, six and twelve days after the removal of the adrenal glands. The explanation of the discrepancy between these results was difficult to find. However, Barrett and Hodges[5] did not use the conventional technique of Sayers, Sayers and Woodbury[6] which employs hypophysectomized rats for the bioassay of adrenocorticotrophin, but a modification of the method described by Hodges[7] in which normal rats pretreated with deoxycorticosterone acetate were used. The possibility exists that the method was not sensitive enough to detect differences between the plasma-levels of adrenocorticotrophin in the normal and adrenalectomized animals or may even have been completely unsatisfactory for the assay of the hormone in blood. Therefore, it seemed essential to repeat and extend the work using hypophysectomized rats for determination of the adrenocorticotrophin.

Blood was collected from normal albino female Wistar rats and from similar adrenalectomized animals at various time-intervals after the removal of their adrenal glands. Plasma was separated and assayed for content of adrenocorticotrophin by the adrenal ascorbic acid depletion method described by Sayers *et al.*[6]. It was found that normal rats exhibited no detectable circulating level of the hormone, neither did animals five days after adrenalectomy. Adrenocorticotrophin was just detectable in the plasma of rats which had been adrenalectomized ten days previously, but its concentration was considerably less than 5 μgm./100ml. plasma. Twenty-three days after adrenalectomy the concentration of the hormone in the plasma had risen to 10·6 μgm./100 ml. (with limits of error ($P = 0·05$) 7·4–15·2 μgm./100 ml.), and the same high circulating level was still present in rats adrenalectomized thirty-four days previously. From the results of this experiment it appears that no rapid and well-maintained increase in the concentration of adrenocorticotrophin in plasma follows adrenalectomy in the rat until three to five weeks after the operation, when there exists a remarkably high circulating concentration of the hormone.

There are no serious discrepancies between these results and those previously reported by Barrett and Hodges[5]. The findings also agree with those of Brodish and Long[8], who demonstrated by a cross-circulation technique that the adrenalectomized rat did not maintain a high circulating level of adrenocorticotrophin until two weeks or more after the removal of the adrenal glands. Furthermore, Barrett and Sayers (Barrett, personal communication) have recently demonstrated similar time-relations in the changes in the concentration of adrenocorticotrophin in the blood of adrenalectomized rats. These results do not contradict the evidence that the release of the hormone from the adenohypophysis may be controlled by the level of adrenal cortical hormones in the blood. However, it appears that in the rat there is an interval of at least two weeks before the pituitary gland adapts itself to secrete *and maintain* an enormously increased output of adrenocorticotrophin in response to the complete absence of circulating corticoids.

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Isomeric Chondroitin Sulphates

A RECENT communication[1] claims to "confirm Meyer's hypothesis that chondroitin sulphates *A* and *C* are only two fractions of the same polydisperse substance". Apparent lack of agreement in nomenclature[2–4] further emphasizes the need for clear differentiation of the isomeric chondroitin sulphates of connective tissue. These three substances, chondroitin sulphate *A*, chondroitin sulphate *B* and chondroitin sulphate *C*, respectively, in the nomenclature of Meyer *et al.*[2], have been found to be linear polyelectrolytes with similar chain configuration-charge relationships. Chondroitin sulphates *A* and *C* have been clearly distinguished as molecular individuals by comparison of molecular constants and physical properties.

Chondroitin sulphate *B* may be readily differentiated from *A* and *C* by enzymatic and chemical techniques[2]. The *A* and *C* sulphates, however, differ only slightly in optical rotation and in solubility, a property also dependent upon molecular weight. These two, however, may be easily differentiated by their infra-red spectra (Fig. 1) in the 700–1,000 cm.$^{-1}$ region where chondroitin sulphate *C* has unique bands at 1,000 cm.$^{-1}$, 820 cm.$^{-1}$ and 775 cm.$^{-1}$. Chondroitin sulphate *A* has bands at 928 cm.$^{-1}$, 852 cm.$^{-1}$ and 725 cm.$^{-1}$; *B* resembles *A*, with bands at 928 cm.$^{-1}$, 855 cm.$^{-1}$, 840 cm.$^{-1}$ and 712 cm.$^{-1}$.

Spectral measurements on fractionated preparations confirm a suggestion[2] that chrondroitin sulphate *C* is present in human umbilical cord. On the basis of published spectra (and optical rotations), the mucopolysaccharide from nuclei pulposi called "chondroitin sulphuric acid-*B*"[3], and the chondroitin sulphate from shark cartilage[7], may also be regarded as the *C* sulphate. In disagreement with a previous report[2], preparations from rabbit skin[8] have been found to consist of a mixture of *A* and *B* sulphates. Small amounts of the *A* sulphate have been detected in samples of β-heparin.

On the basis of previous assignments of bands[3], it appears that the sulphate group has an axial configuration in chondroitin sulphate *A* and *B* and an equatorial configuration in sulphate *C*. In agreement with recent reports[9] a possible interpretation places the sulphate group on the C-4 position in the hexos-

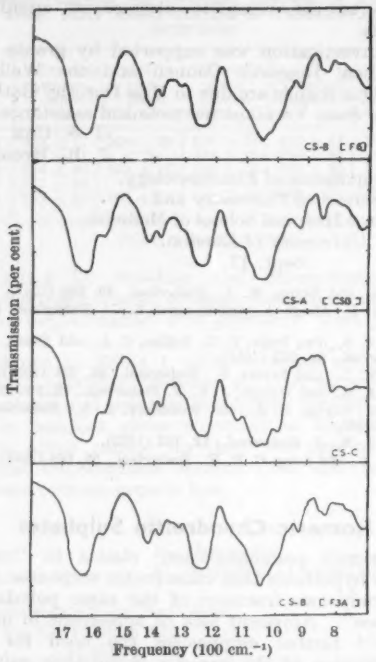

Fig. 1. Infra-red spectra for isomeric chondroitin sulphates obtained on a Perkin–Elmer spectrophotometer, model 21, with sodium chloride prism. 1·0 mgm. of the sodium salt of each polysaccharide was mixed with 150 mgm. potassium bromide, formed into a film of 0·45 mm. thickness (nominal) and compared with a potassium bromide blank. *A* was prepared from bovine nasal cartilage (ref. 5). Chondroitin sulphate *B* preparations were obtained by fractionation of a sample of β-heparin from bovine lung (ref. 6). One of these fractions, *F*-6, contained 20 per cent of the *A* sulphate. The *C* sulphate was a preparation from a human chordoma (ref. 2)

amine moiety of both the *A* and *B* sulphate, and on the C-6 position of the hexosamine moiety of the *C* sulphate.

This work, a fuller account of which is in preparation, was supported by a grant from the Chicago Heart Association and carried out during the tenure of an established investigatorship of the American Heart Association. I am indebted to Mr. Robert Hart for the spectral measurements, to Dr. A. Winterstein for the preparation of β-heparin, and to Dr. K. Meyer for the chondroitin sulphate *C* preparation.

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Effect of Hypophysectomy on the Ability of Rat Liver Microsomes in a Cell-free System to incorporate Radioactive Amino-Acids into their Proteins

The incorporation of DL-leucine labelled with carbon-14 into the proteins of a cell-free system[1] prepared from rat liver has been compared with the incorporation obtained under the same conditions with a system prepared in exactly the same way from the livers of hypophysectomized rats. The system was prepared by homogenizing liver in a buffered-sucrose medium[1] and spinning at 14,000*g*. The unsedimented portion was incubated with the sodium salt of adenosine triphosphate, the sodium salt of creatine phosphate and DL-leucine-1-^{14}C. The incorporation obtained varied with the experimental conditions used, but in all cases less amino-acid was incorporated into the protein of hypophysectomized rats. The specific activity of the protein from hypophysectomized rats in ten experiments was between 20 and 70 per cent of that found in the normal control system. Treatment of the hypophysectomized rats with growth hormone restored almost to the normal level the ability of the cell-free system to incorporate amino-acids. This *in vitro* result is compatible with the *in vivo* observations that hypophysectomy results in a fall in liver weight and protein content and that treatment with growth hormone prevents this fall[2].

Incorporation into the proteins of the cell-free system takes place in at least two distinct steps. Hoagland, Zamecnik and Keller[3] have shown that the first step is the activation of the amino-acid by adenosine triphosphate and an enzyme present in the soluble fraction of the cell. Later the carboxyl-activated amino-acid is incorporated into protein on the ribonucleoprotein particles of the microsomes[4,5]. There may be intermediate steps between these two processes, and Hoagland, Zamecnik and Stevenson[6] have some evidence which implicates part of the ribonucleic acid of the soluble fraction of the cell in such a role.

The microsomes were separated from the activating enzymes and ribonucleic acid in the soluble fraction of the cell by centrifugation of the cell-free system at 105,000*g*. Neither the microsomes nor the soluble fraction when incubated separately in the presence of adenosine triphosphate, creatine phosphate and DL-leucine labelled with carbon-14 showed much incorporation, but incorporation into microsomal protein, to the extent found in the unseparated system, occurred when these two fractions were incubated together. A similar experiment (Table 1) with microsomes and soluble fraction from hypophysectomized rats showed the expected smaller incorporation into these microsomes. The microsomes from hypophysectomized rats were now incubated with the soluble fraction (supplemented with adenosine triphosphate and creatine phosphate) prepared from the normal animals and almost the same, smaller, incorporation was found. When normal microsomes were incubated with soluble fraction from hypophysectomized rats, almost normal amounts of radioactive amino-acid were incorporated into the microsomes. This shows that hypophysectomy was not causing changes in the activating enzymes, the energy generating system, or the possible ribonucleic acid intermediate[6] to any great extent, for the soluble fraction from hypophysectomized rats is capable of

Table 1. INCORPORATION OF DL-LEUCINE LABELLED WITH CARBON-14 INTO MICROSOMAL PROTEIN OF A CELL-FREE SYSTEM

	c.p.m./mgm. of microsomal protein
Normal microsomes + normal soluble fraction	146
Hypophysectomized microsomes + hypophysectomized soluble fraction	76
Hypophysectomized microsomes + normal soluble fraction	87
Normal microsomes + hypophysectomized soluble fraction	138

4 mgm. of microsome protein and 6 mgm. of soluble fraction protein were incubated at 37° in a buffered-sucrose medium (ref. 1) at *p*H 7·5 with adenosine triphosphate (6×10^{-3} *M*), creatine phosphate ($2{\cdot}3 \times 10^{-2}$ *M*) and DL-leucine-1-^{14}C ($1{\cdot}3 \times 10^{-4}$ *M* and about 40 μc./mgm.).

causing almost as great an incorporation as the soluble fraction from normal rats if it is incubated with normal microsomes. Most of the fall in incorporation resulting from hypophysectomy must be caused by some deficiency of the microsomes themselves. It is well known[7,8] that hypophysectomy in the rat is followed by a fall in the cytoplasmic ribonucleic acid content of the liver which can be prevented by administration of growth hormone. The observed changes in incorporating ability do not, however, depend on the total ribonucleic acid content of rat liver. Clark, Naismith and Munro[9] have shown that a large fall in the ribonucleic acid of rat liver occurs when rats are fasted, yet I have found incorporation of amino-acid into microsomes of cell-free systems from both normal and hypophysectomized rats fasted for 18 hr. to be the same as in fed animals. The ribonucleic acid which is broken down as a result of 18 hr. of fasting is not the same ribonucleic acid which is responsible for amino-acid incorporation into protein, nor is it the same as the ribonucleic acid which is lost as a result of hypophysectomy.

These experiments do not exclude the possibility that hypophysectomy also influences protein synthesis by inhibiting the entry of amino-acids into the cell or by decreasing the amount of energy available for these processes or by some other mechanism. It is, however, clear that hypophysectomy markedly reduces the ability of rat-liver microsomes to incorporate activated amino-acids into protein. The depression of protein synthesis after hypophysectomy[2] is probably secondary to this effect on the microsomes.

It is hoped to publish full details of these experiments in due course. I wish to express my thanks to Prof. F. G. Young for his interest and encouragement, to the Council of the Royal Society for a grant-in-aid, to Miss M. B. Thomas who performed the hypophysectomies and to Mr. D. C. Gardiner for highly skilled assistance.

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Adrenaline and Noradrenaline Content of the Adrenal Gland of the Rat following Depletion with Reserpine

IT has been shown that reserpine causes a loss of catechol amines from cat[1] and rabbit[2] adrenal glands. In the experiments reported here a study has been made of the adrenaline and noradrenaline content of rat adrenal glands over a period of a month, reserpine being administered for the first 3 days. Thus information concerning the loss and replacement of amines was obtained.

Albino rats weighing 150–190 gm. were used, and in any one experiment all were of the same sex. In all instances groups of 5 rats were used, and every group had its own control, also of 5 rats. Each test animal was given reserpine (1 mgm./kgm.) subcutaneously, daily for three consecutive days. The control rats were given the reserpine solvent only. Groups of rats, with their respective controls, were killed at 3, 7, 14, 21 and 28 days following the first injection. In all, 170 rats were used. The adrenaline and noradrenaline contents of extracts of the glands were determined biologically, using the blood pressure of the hexamethonium-treated cat and the isolated uterus of the rat in dioestrus as test preparations. The results were calculated by the formula of Bülbring[3] and expressed in μgm./gland.

It was found that, at 3 days, reserpine caused a similar percentage loss of adrenaline and noradrenaline. This was despite the fact that normal rat adrenal glands contain only 10 per cent of noradrenaline. In all experiments the degree of depletion of both amines was of the order of 50 per cent. Following the depletion, the adrenaline content of the glands slowly returned to its initial, non-depleted value by 21 days and afterwards stayed at this level. In contrast, noradrenaline increased to several times its original value by seven days and then decreased to regain its original value by 21 days. Fig. 1 shows the typical percentage depletion of amines and their subsequent replacement. It may be noted that at the peak of the noradrenaline increase, the glands contained approximately 30 per cent of noradrenaline. All experiments gave similar results, there being no difference in this respect between male and female rats.

These results are further evidence that noradrenaline is a precursor of adrenaline in the rat, because otherwise it would be expected that the two amines

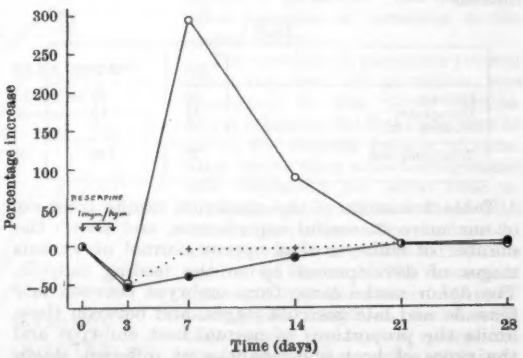

Fig. 1. A composite diagram to show the rate of replacement of adrenaline (●), of noradrenaline (○) and of total amine (+) in the adrenal gland of the rat, at various time intervals following depletion with reserpine. All results are expressed as percentages of the amine contents of the respective control glands

would return to their normal levels in a similar manner. The transient increase in the noradrenaline content, above the normal value, indicates that the rate of methylation of noradrenaline to adrenaline is slower than the rate of formation of noradrenaline. It is of interest that Butterworth and Mann[4] have obtained a similar depletion and replacement of catechol amines in cat adrenal glands, depleted with acetylcholine. Work is now in progress to determine whether this action of reserpine is central or peripheral.

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Nuclear Transplantation in *Xenopus laevis*

Briggs and King[1] have devised a technique with which they were able to transplant nuclei from cells of early embryos into unfertilized eggs of *Rana pipiens* from which the female pronucleus had been removed. In this way they could test the extent to which a nucleus from embryos up to neurula stages could produce normal development when transplanted into an enucleated egg[2].

We have modified this technique to start similar experiments on *Xenopus laevis*. We have not so far removed the female pronucleus because it does not usually participate in development. This useful fact was discovered through the use of a nuclear marker, visible in interphase nuclei (Fischberg, M., Smith, S., and Elsdale, T., unpublished).

A survey of the number of chromosome sets present in 70 normal and abnormal tadpoles gave the results shown in Table 1. The tetraploid embryos are usually due to the doubling of the diploid chromosome number of the injected nucleus, and not to the participation in development of the female pronucleus[1].

Table 1

	No.	Per cent
Diploids	55	79
Tetraploids	12	17
Mosaics, etc.	3	4
Total analysed	70	100

Table 2 consists of the combined results from six of our more successful experiments, and shows the number of embryos that appear normal at various stages of development up to the feeding tadpole. The donor nuclei came from embryos between late blastula and late gastrula stages, and between these limits the proportions of normal host embryos and the types of host abnormalities at different stages are similar; these appear not to be dependent upon the developmental stage of the donor nucleus and the germ layer from which it derives. Table 2 is

Table 2

	No.	Per cent
Total transfers	905	100
Uncleaved	387	43
Cleaved normal and abnormal	518	57
Normal gastrulæ	193	21
Normal at hatching	89	10
Normal feeding tadpoles	61	7

heterogeneous, because it consists of the results from twelve different categories of donor nuclei (four temporal stages and three germ layers); we find that the numbers in the individual categories are too small to relate the abnormalities of transplanted embryos to the origin of the donor nucleus.

We have obtained normal tadpoles from eggs injected with ectoderm and endoderm nuclei from late gastrulæ. King and Briggs[2] obtained 10–12 mm. tadpoles from nuclei of ectoderm, chorda-mesoderm, and endoderm from late gastrulæ of *Rana pipiens*; but the majority of transplants from chorda-mesoderm and endoderm gave embryos developing syndromes of abnormalities specific for the origin of the donor nucleus.

We are continuing the work along two main lines: (*a*) to find the latest stages that provide donor nuclei which give normal development when transplanted into unfertilized eggs; (*b*) to see if embryos develop syndromes of abnormalities specific for the origin of the donor nucleus.

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Effects of Heat and Loss of Moisture on the Dormancy of Barley

In a paper on the effects of desiccation on delayed germination in barley it is suggested that ripening and germination are directly related to the moisture-content of the grain[1]. The observation is made that complete germination occurred when the moisture content naturally fell to 12·7 per cent or when artificial drying was used.

'Desiccation' was the word employed to describe drying at 40° C. It is the purpose of the present communication to direct attention to the fact that temperature can have an effect independent of the loss of moisture of harvested samples showing dormancy under seed testing conditions[2].

In an attempt to speed germination, treatments were given which included heat-sealing in polythene bags, some of which contained self-indicating silica gel, and oven-drying at 39° C. Room temperature, in late summer, varied between 16–24° C. Moisture-content was determined with a Marconi 933*A* meter, and checks made with a Carter/Simon oven. From each treatment 300 seeds were placed on damp sand.

Samples of Pioneer barley *A*27 (moisture content 16·3 per cent and germination 95 per cent), known to be dormant were sealed in polythene with silica

gel. Overnight the moisture-content fell to 12·5 per cent when kept at 4° C. and to 10·7 per cent at room temperature. Germination after 6 days reached only 60 per cent and 57 per cent respectively; not significantly different from the control at 51 per cent.

At the same time samples of *A*27 were placed at 39° C. for 4 days. Germination after 6 days reached 95 per cent whether the sample was sealed (with a moisture loss of ≥ 1 per cent) or had dried down to 8·4 per cent moisture. The control, kept in a closed but not sealed polythene bag at room temperature for the 4 days, grew 75 per cent in 7 days.

Similar treatment was given to barley *A*29, moisture content 17·5 per cent, germination without treatment 60 per cent. After 2 days (or 4 days) at 39° C. germination reached 95 per cent or more with moisture-losses of either ≥ 1 or 7·5 per cent. Germination was adversely affected when grains on damp sand were sealed in polythene at 39° C. for 2 days.

It is suggested that, during natural desiccation, temperature, and probably time, may be more important factors than the precise moisture-content in affecting this type of delayed germination.

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A Mechanism of Resistance to Parathion in *Musca domestica* (L.)

The ease with which the chlorinated hydrocarbons give rise to the development of resistance in *Musca* has led to the use of organo-phosphorus insecticides for controlling flies. However, development of resistance to some of these compounds appears possible also, although at present it seems to be less serious than in the case of the chlorinated compounds. An investigation into the mechanisms causing resistance to these substances has been undertaken.

Three strains, namely *S*, U_2 and *D*, with different parathion resistances were used in the present investigation. *S* is our susceptible laboratory strain which is also susceptible to parathion. Strain U_2, being highly resistant to γ-BHC and related compounds, shows some resistance to parathion, although it has never been in contact with organo-phosphorus insecticides. Strain *D* was collected by Keiding[1] on a farm (*J*79) in Denmark, where parathion had been used for a period of three years. It was submitted to further selection with parathion in the laboratory. Dr. Keiding kindly put this strain at our disposal.

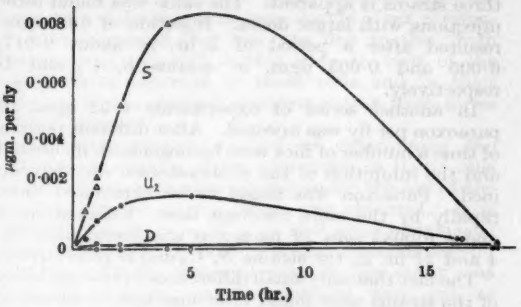

Fig. 2. Amount of paraoxon found in flies after injection of 0·01 μgm. of parathion in strains *S*, U_2 and *D*

It is assumed that parathion is oxidized in the flies to paraoxon, just as is the case in mammals[2] and in the roach (*Periplaneta americana*)[3], and that this compound exerts its action by inhibition of an enzyme, probably cholinesterase. As a first step, the susceptibilities of the strains to parathion and paraoxon were compared, using a contact method and injections (Fig. 1). In the contact experiments the flies were exposed in jars for twenty-four hours to a residue in a thin film of peanut oil. For the injections an emulsion of parathion dissolved in peanut oil was used, whereas paraoxon was injected as a solution in insect saline.

It was found that there is still a marked difference between the strains after injection of parathion. Therefore decreased penetration cannot be the cause of the resistance, or at least not the only cause. The differences found in the contact toxicity of paraoxon indicate that a resistance mechanism must be present that affects the susceptibility to parathion only through an influence on the toxic action of paraoxon. The difference after injection of paraoxon is discussed below.

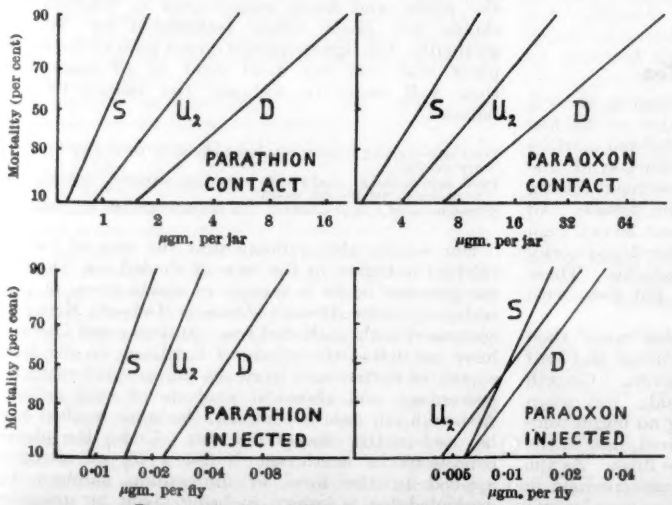

Fig. 1. Dosage-mortality curves of strains *S*, U_2 and *D*

The amount of paraoxon present after injection of parathion was estimated in the three strains. Upon injection the flies were kept at 25° C. for varying periods of time, after which they were homogenized and incubated for some time to bring the paraoxon in contact with the cholinesterase. The cholinesterase activity was estimated by the method of Hestrin[4]. The amount of paraoxon was determined by comparing the inhibition found with that caused by known quantities. Fig. 2 shows the results of injecting 0·01 μgm. of parathion per fly. A very large difference between the amounts of paraoxon in the

three strains is apparent. The same was found after injections with larger doses. Injection of 0·02 μgm. resulted after a period of 5 hr. in about 0·017, 0·005 and 0·003 μgm. in strains *S*, U_2 and *D* respectively.

In another series of experiments 0·01 μgm. of paraoxon per fly was injected. After different periods of time a number of flies were homogenized, incubated and the inhibition of the cholinesterase was determined. Paraoxon was found to be eliminated more rapidly by the more resistant flies. Elimination of about 0·0085 μgm. of paraoxon was found after $6\frac{3}{4}$, 4 and $2\frac{3}{4}$ hr. in the strains *S*, U_2 and *D* respectively.

The fact that only small differences in susceptibility of the strains were found after injection of paraoxon may be explained by the very rapid action in this case. Apparently the elimination process can only protect the flies from the action of paraoxon if it appears in the body gradually.

The occurrence of only small differences in susceptibility after injection of paraoxon makes it less probable that an alteration of the affected enzyme is a cause of the resistance. As cholinesterase may possibly be this enzyme we estimated its activity in the strains and the amount of paraoxon required for its inhibition *in vitro*. Neither quantitative nor qualitative differences were found which could contribute to the resistance.

It is remarkable that strain U_2 —which was selected with γ-BHC —eliminates paraoxon much faster than strain *S*. This difference is not incidental, for it was found that strain *U*, a substrain of U_2 that has been bred for some years without selection and has lost its resistance to γ-BHC, has the same susceptibility to parathion as strain *S* and about the same capacity to eliminate paraoxon. There seems to be in this case some relation between the resistances to these quite different types of insecticides.

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Boron Deficiency in Tea

Tea must be the last commodity crop to show a need for boron. This is probably due to its low calcium requirement and to the fact that tea gardens are very rarely limed. The boron deficiency syndrome first appeared accidentally in a pot experiment testing five different soils for natural potassium uptake. All plants in two soils when they were about 30 cm. high died off at their growing points and developed corky excrescences on the undersides of petioles. These two soils were known from previous pot tests with tomatoes to be deficient in boron.

Half the plants in the experiment were then treated with a complete nutrient solution and half with the same solution without boron. Growth response to each treatment was rapid; but when the new shoots of the plants receiving no boron were about 25 cm. long, brown spots appeared, first in the petioles and then in the laminæ of the flush. As the young leaves developed, the brown spots enlarged in the laminæ and spread along most of the petioles and mid-ribs with considerable suberization. Normal growth was severely upset and affected leaves were strongly wrinkled and contorted. Cork was particularly thick on the underside of the petioles and often spread to the adjacent stems. Laminæ of affected petioles drooped to a near vertical position but remained quite turgid. Internodes were reduced to less than 1 cm. and finally growing-points died. Old leaves that had reached maturity before the boron deficiency became acute, exhibited suberized cracks on both sides of the upper surface of the main veins.

So far as can be ascertained the above has not yet been observed in field-grown tea bushes.

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Rate of Calcium Loss resulting from Ammonium Sulphate Treatment of a Tea Soil

Full results of our investigation into the effect of treatment with ammonium sulphate on the calcium economy of a tea soil will be published elsewhere[1]; but certain preliminary observations can now be made. The soil under study, which is typical of the tea soils of north-east India, can be described as a highly acid loamy sand of alluvial origin, having *p*H about 4·5 and a low calcium content.

It is generally assumed[2] that the loss of bases resulting from application of 100 lb. of ammonium sulphate can be made good by applying about 110 lb. of calcium carbonate to the soil. The calculated rate of loss under our field conditions is, however, only about a third of the above figure. Our results appear to provide an indirect proof that tea growing in an acid soil and manured with ammonium sulphate absorbs nitrogen in the 'ammonia' form. It also follows from the results that, under our conditions, the tea plant takes up a considerable portion of its nitrogen requirement as 'free nitric acid'. The theoretical basis on which this latter deduction is made has already been explained by Pierre[3]. A chemical analysis of the tea plant growing in an acid soil supports the above deductions. As an illustration, the acidic and basic constituents in plucked tea shoots are given below (calculated by Truog's method[4]). The figures quoted do not refer to the whole plant and will not hold good in all cases, but they will serve to indicate the nature of the balance.

Total base-forming constituents, approximately 90 m.equiv./100 gm. dry weight.
Total acid-forming constituents excepting nitrogen, approximately 38 m.equiv./100 gm. dry weight.
Nitrogen content, approximately 875 m.equiv./100 gm. dry weight.

Our results also indicate that the rate of loss of calcium is higher in the case of shaded tea (that is, tea growing under a canopy of shade trees, in this instance, under *Albizzia chinensis* (Osbeck) Merr.) as compared with unshaded tea. Andrews and Cowart[5] have calculated the effects of fertilizers on the base supply of cotton soils from the unrecovered nitrogen percentage and chemical analysis of seed cotton. Although our case is complex, the same method can be used in the case of tea also. Under the plains conditions in north-east India, 1 lb. of nitrogen applied in the form of ammonium sulphate to unshaded tea is known to bring about an ultimate

increase in yield of about 8 lb. of made tea. The corresponding increase in the case of shaded tea, on the other hand, is only about half that amount. From the nitrogen content of plucked shoots, an idea can be obtained about the percentage nitrogen unrecovered, although not all of it may necessarily be lost by leaching. It would appear that a higher percentage of nitrogen remains unrecovered in the case of shaded tea, and this, it is suggested, may serve as a qualitative explanation for the higher rate of calcium loss noted in the case of shaded tea.

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Occurrence of Sex Chromatin in the Basal Cells of the Epidermis and in Basal Cell Carcinomata

THE characteristic mass of sex chromatin, first observed by Barr and Bertram[1] in the nerve cells of the female cat, can be identified in the cells of several different tissues in the human female[2]. The method of 'nuclear sexing' is now a well-recognized technique of exceptional importance in the investigation of disorders of human sexual development. The technique has been applied to the study of human tumours and, provided that good histological preparations have been available, it has been found, in most cases, that sex chromatin was recognizable in the cells of tumours of female patients but not in those of males, that is, the tumour is of the same sex as the host[3–5]. The principal exception is in the case of teratomata, which are always of female nuclear sex if the host is female, but may be male or female if the host is male[3–6]. Two alternative hypotheses have been postulated in explanation of this anomaly: one presumes that the tumour arises by parthenogenesis of haploid germ cells followed by endomitosis to restore the diploid chromosome number; the other supposes that the tumour arises by conjugation of two haploid germ cells.

In addition to teratomata the basal cells of the epidermis and the basal-cell tumours of the skin have been found to be exceptional in this respect. Weinmann, Meyer and Marwah[7] found difficulty in diagnosing the nuclear sex of basal cell tumours of the skin. In a series of 11 such tumours from male patients a sex chromatin-like particle could be identified in many of the parenchymal neoplastic cells and also in many of the cells of the adjacent normal epidermis. It was not suggested that this particle indicated a sex reversal in these tumours, but rather that the chromatin structure of the cell was altered during oncogenesis. A similar explanation was invoked by Tavares[8]; he examined "undifferentiated cell carcinomata", a group of tumours regarded by him as separate from those showing evidence of differentiation, and found that of 18 tumours in males two appeared to be female; while of 35 tumours in females six appeared to be male and in 18 instances the result of sexing the tumour was equivocal. Sachs and Danon[9], on the other hand, reported that the basal cells of the epidermis were devoid of sex chromatin and suggested that this was due to the "high frequency of mitosis" in this site. In view of the discrepancies reported in these cells and in their tumours it was decided to investigate a fresh series of specimens.

The most critical method of determining the presence or absence of sex chromatin in tissue cells is to use these cells alone to determine the sex of the patient from whom the tissue was obtained. It is essential for this type of work that good histological preparations and suitable staining methods should be used. All the material used in this study was fixed in formalin, paraffin sections were cut at 5μ and were stained in hæmatoxylin and eosin, the staining being controlled by microscopical examination during differentiation.

A series of 37 sections of skin taken from the routine surgical material of the hospital was examined. The 'nuclear sex' was determined, first by examination of 100 basal cells and secondly by examination of 100 spinous cells of the overlying epidermis. Nineteen of the patients were female and eighteen male, in every case the 'sex' of the basal cells, the 'sex' of the spinous cells and the apparent morphological sex of the patient were congruous.

A consecutive series of 24 basal cell carcinomata were examined. The sections were first reviewed under the low power of the microscope to confirm the histological diagnosis and to select an area of tumour in which the cells were not too closely packed together by the surrounding tissues. The 'nuclear sex' was then determined by examination of 100 tumour cells followed by the examination of 100 cells in the adjacent epidermis. Nine of the patients were male and fifteen female, in every case the 'sex' of the tumour, of the skin and of the whole patient were identical.

Repeated examinations were made of many of the slides at different times; in no instance were anomalous results obtained.

These observations indicate that there is no peculiarity in the basal cells nor in their tumours, and that, given good histological preparations, sex chromatin can be identified in these structures as in all others. This point was made by Sohval and Gaines[10], who examined 134 tumours from female patients and found sex chromatin in only 33, and is of the utmost importance especially when cytological examination of cells and cell nuclei is used in the investigation of cases of sexual abnormality.

I am indebted to Miss C. D. M. McDougall and Mr. S. E. Wright, who were responsible for the excellent histological preparations which made this work possible.

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Effect of 2,4-Dichlorophenoxyacetic Acid on Pectin Methylesterase in Tobacco Pith Sections

The hypothesis by Bennet-Clark[1] that the degree of methyl esterification of the pectic substances of plant cell walls would have a marked effect on the plastic and elastic properties of such walls has directed attention to the role of the pectic substances in auxin-controlled extension growth. Ordin, Cleland and Bonner[2] have demonstrated that the methylation rate of pectins in the walls of coleoptiles is increased by auxin, and Tagawa and Bonner[3] have shown that the effect of various cations and of auxin on the physical properties of the walls are consistent with Bennet-Clark's hypothesis.

Recently, it has been shown[4,5] that the auxins, indoleacetic acid, 2,4-dichlorophenoxyacetic acid and α-naphthalene acetic acid may control the adsorption of pectin methylesterase to cell-wall fractions prepared from tobacco pith or Jerusalem artichoke tubers. The auxins, up to an optimal concentration of about 10^{-10} *M*, increased the amount of enzyme adsorbed. It was proposed that the extent of methyl esterification of the pectic substances of the walls in elongating cells could be controlled by auxin-induced adsorption of pectin methylesterase.

Evidence has now been obtained for an auxin-controlled mechanism of adsorption of pectin methylesterase in intact disks of tobacco pith. When pith from *Nicotiana tabacum* v. *glauca* was placed in a solution of 0·5 per cent pectin and 0·1 *M* sodium chloride, considerable pectin methylesterase activity could be measured. The rate of hydrolysis of ester bonds of the pectin (measured by the rate of acid production) gradually increased until a steady rate was reached. Vacuum infiltration of the disks with the pectin assay solution did not reduce the time for the attainment of the steady rate, indicating that the initial change of rate was not due to penetration of pectin into the disks.

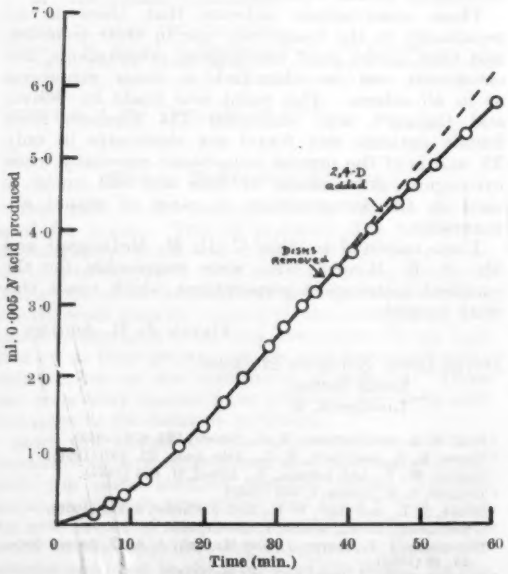

Fig. 1. Hydrolysis of pectin by pectin methylesterase extracted from tobacco pith disks by the assay solution. (0·5 per cent pectin; 0·1 *M* sodium chloride)

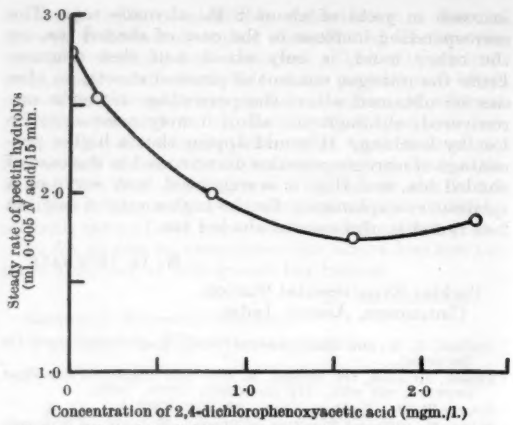

Fig. 2. Effect of 2,4-dichlorophenoxyacetic acid on the extraction of pectin methylesterase from tobacco pith disks. (0·5 per cent pectin; 0·1 *M* sodium chloride)

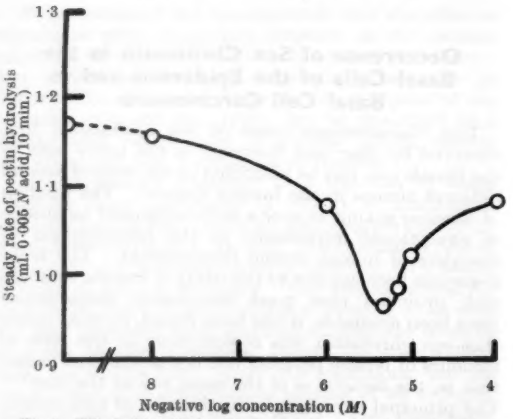

Fig. 3. Effect of 2,4-dichlorophenoxyacetic acid on the extraction of pectin methylesterase from tobacco pith disks. (0·5 per cent pectin; 0·2 *M* sodium chloride)

Removal of disks from assay solution after the steady rate had been reached showed that about 90 per cent of the measured pectin methylesterase activity remained in solution. When sodium chloride was omitted, no pectin methylesterase was extracted.

If disks which had been in pectin and sodium chloride until a steady rate of acid production was reached were transferred to a fresh assay solution, a second lot of pectin methylesterase was extracted. Again a steady rate was reached. The steady rate, therefore, represented an equilibrium position between enzyme in the assay solution and enzyme bound in the disks. The enzyme in the disks did not contribute substantially to the rate at which the pectin of the assay solution was de-esterified. This can be seen from Fig. 1, which also shows that 2,4-dichlorophenoxyacetic acid had no significant effect on the activity of pectin methylesterase when free in solution.

When 2,4-dichlorophenoxyacetic acid was included in the assay system, the steady rate of pectin hydrolysis by the disks was greatly affected. In Fig. 2 the steady rates are plotted against concentration of 2,4-dichlorophenoxyacetic acid. Fig. 3 shows results for a wide range of concentrations of 2,4-dichlorophenoxyacetic acid and 0·2 *M* sodium chloride as the extracting solution.

It is suggested that 2,4-dichlorophenoxyacetic acid shifts the equilibrium position for partitioning the enzyme between the disks and the external medium by controlling the binding of pectin methylesterase to sites in the cell wall.

These observations strengthen the hypothesis for the relation between auxin-controlled extension growth and auxin-controlled adsorption (inactivation) of pectin methylesterase[4,5]. Evidence for the heterogeneity of pectin methylesterase and adsorption sites in cell walls, a mathematical treatment of the adsorption process, and full details of experiments outlined here will be published elsewhere.

Acknowledgments are due to Dr. R. N. Robertson for helpful advice and criticism and to Miss Sue Ingles for technical assistance.

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Radicle Elongation of *Pseudotsuga menziesii* in Relation to Light and Gibberellic Acid

One of the characteristics of the 'bakanæ' disease of rice (which is caused by *Gibberella fujikurio*, the fungus which produces gibberellic acid) is its ability to stimulate adventitious root formation. Attempts to increase root development with artificially supplied gibberellic acid, however, have almost invariably failed; indeed, it has been claimed that root elongation is inhibited by the gibberellins[1,2]. With few exceptions, observations of root effects have been incidental to studies of shoot development and it is possible, therefore, that observed effects on roots are indirect and result from a re-distribution of material within the plant from root to shoot.

In view of the uncertainty relating to the influence of gibberellic acid on root elongation, it seemed of interest to test its effect directly, on material uncomplicated by the presence of an active shoot system. In the experiments reported here, elongation of the radicles emerging from germinating Douglas fir seed before the onset of cotyledon growth was used to measure the effect of the chemical. At the same time, a possible interaction between gibberellic acid and light was investigated.

Preliminary tests employed a light treatment (continuous light at 4,000 lux and continuous darkness) and five gibberellic acid treatments (0, 3, 10, 30 and 100 p.p.m. applied in de-ionized water to filter paper and used as a growth medium). Root elongation over a 24-hr. period was markedly inhibited by exposure to light, but in both light and darkness there was a stimulation by low concentrations (about 3 p.p.m.) of gibberellic acid. To investigate these effects more precisely, seeds were germinated at 20° C. in Petri dishes and treated with gibberellic acid in concentrations of 0, 0·01, 0·10, 1·00 and 5·00 p.p.m. Again, two light treatments (continuous light and continuous darkness) were employed. Radicle elongation-rate over a 24-hr. period was measured on 45 roots from each treatment, chosen so that three size classes (<5 mm., 5–10 mm. and >10 mm. in initial length) were equally represented.

In Fig. 1, mean growth-rates are plotted against acid concentration by size class and light treatment. Roots greater than 5 mm. in initial length show a marked response to gibberellic acid treatment, the optimum concentration shifting from 10 p.p.m. to 5 p.p.m. with an increase in initial length of root.

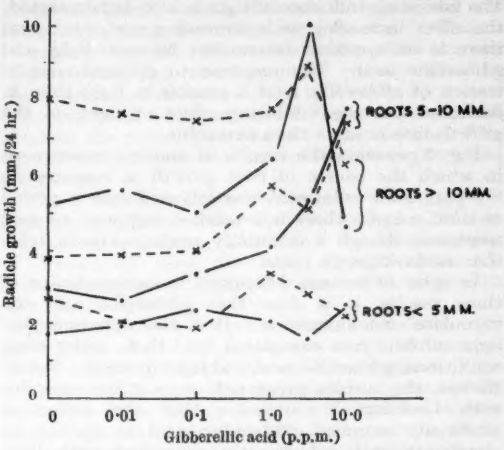

Fig. 1. The effect of light and gibberellic acid on radicle elongation of *Pseudotsuga menziesii* as related to initial root length. × - - - ×, Growth in darkness; • — •, growth in light

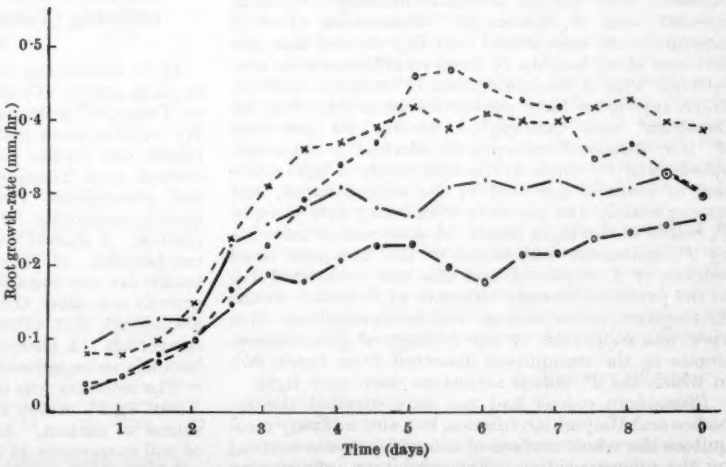

Fig. 2. The effect of light and gibberellic acid on the time course of radicle elongation in *Pseudotsuga menziesii*. • — •, dark; × - - ×, dark + gibberellic acid; ○ — ○, light; ○ - - ○, light + gibberellic acid

The inhibiting influence of light is also demonstrated, the effect increasing with increasing root size; and there is an apparent interaction between light and gibberellic acid. The response to optimal concentration of gibberellic acid is greater in light than in darkness and the inhibiting effect of light on the growth-rate is more than overcome.

Fig. 2 presents the results of another experiment in which the course of root growth in response to 8 p.p.m. gibberellic acid was followed over a period of time. Again there is a marked response to acid treatment though it is initially much greater in light- than in dark-grown roots.

In spite of certain difficulties in interpretation of these results, it is clear that gibberellic acid can stimulate root elongation. It is also apparent that light inhibits root elongation, and that, under some conditions, gibberellic acid and light interact. Nevertheless, the results presented are not incompatible with Lockhart's[3] contention that the action of artificially supplied gibberellic acid is specific on elongation and not directly connected with light responses. With the view of analysing the light inhibition of root elongation, studies of its action spectrum have been initiated. There are preliminary indications that the light-reaction peaks at 5400–5800 A. with subsidiary peaks at 3800 A. and 6600 A. A full report on this work will be published elsewhere.

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Partial Inhibitory Effect of *Plistophora culicis* on the Sporogonic Cycle of *Plasmodium cynomolgi* in *Anopheles stephensi*

During some experimental work carried out on the cytology of early oocysts of *Plasmodium cynomolgi* in *Anopheles stephensi* an interesting observation was made. Three batches of laboratory-bred *Anopheles stephensi* were fed on a rhesus monkey (*M.* 172) infected with *P. cynomolgi*. Dissections of 4–10 mosquitoes on each subsequent day showed that the first and third batches of these mosquitoes were also infected with a microsporidian (*Plistophora culicis*). (Such infections have previously been described by Garnham[1] and Canning[2].) Nearly 50 per cent of the dissected mosquitoes showed a moderate infection of *P. culicis* in the first batch, a light infection of about 5 per cent in the second batch, and approximately 100 per cent with heavy infections of *P. culicis* in the third batch. A poor rate of infection by *P. cynomolgi* was found in the first and third batches of *A. stephensi*, and this was considered due to the partial inhibitory influence of *P. culicis* on the development of the oocysts in these mosquitoes. This view was supported by the findings of good oocysts counts in the mosquitoes dissected from batch two in which the *P. culicis* infections were very light.

Plistophora culicis had not only invaded the fat bodies and Malpighian tubules, but also in many mosquitoes the whole surface of the mid-gut was covered by the microsporidian. The inhibitory influence on the development of *P. cynomolgi* in such insects was primarily reflected in a low oocyst count; in most mosquitoes only one or two oocysts could be found on each mid-gut. The growth of these oocysts was also retarded. Thus five-day-old oocysts measured 19μ in diameter compared with the normal oocyst size 21μ seen in mosquitoes uninfected with *P. culicis* after this time. Seven-day-old oocysts measured only 55μ in diameter compared with the normal size of 60μ. The diameter of mature oocysts in the mid-guts heavily infected with *P. culicis* was only 84μ compared with the usual 90μ. The highest rate of infection with *P. cynomolgi* oocysts in the mosquitoes concomitantly infected with *P. culicis* was 20 per cent and the lowest 10 per cent. Many degenerating oocysts were seen.

One may conclude that the partial inhibitory effect of *Plistophora culicis* on the sporogonic cycle of *P. cynomolgi* in these batches of *A. stephensi* may be due to the following possibilities: (1) The scarcity of essential nutriments in the body of the host may be responsible for the retarded growth and degeneration of fully developed ookinetes and young oocysts. These essential substances were presumably taken up by *P. culicis* for its own development. (2) Many specimens devoid of oocysts showed the whole surface of the mid-gut covered with *P. culicis*. The presence of this microsporidian may have prevented the penetration of ookinetes through the stomach wall and resulted in the degeneration of these 'vermicules' inside the lumen. (3) The heavy infection of Malpighian tubules with *P. culicis* may cause abnormality in their function. Afterwards the deposition of toxic substances might interfere with the development of oocysts and sporozoites of *P. cynomolgi* in the insect host.

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Isolation of a Soil Coccus capable of utilizing 'Tween 80' as a Sole Source of Carbon

It is interesting that some organisms can utilize as their source of carbon a synthetic substance such as 'Tween 80' (polyoxyethylene sorbitan mono-oleate). My collaborators and I[1] have proved that tubercle bacilli can utilize 'Tween 80' as a sole source of carbon, and Yamane *et al.*[2] then devised a simple and practicable 'Tween'-agar medium for tubercle bacilli containing 'Tween 80' as a main source of carbon. I wished to investigate[3] the details of the mechanisms of 'Tween' metabolism, but tubercle bacilli are not suitable for this purpose because their growth is so slow, their endogenous substrates are too abundant[4], and experiments with the bacilli can be dangerous. I therefore decided to try to isolate bacteria more suitable for the purpose from the soil.

The isolation was carried out on Yamane–Minami–Yasui agar[1], which contained 'Tween 80' as a sole source of carbon. After incubating the supernatant of soil suspension at 37° C. for 24 hr., a few bacterial colonies were developed, among which there was a coccus. This organism was a Gram-negative coccus

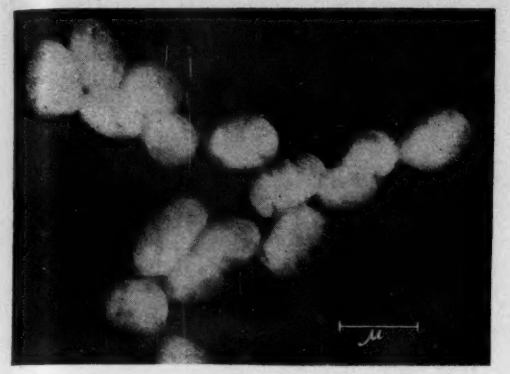

Fig. 1. Electron micrograph of the newly isolated coccus grown in a synthetic medium containing 'Tween 80' as a sole source of carbon for 6 hr. Shadowed with chromium. Flagella-like structures around the cell periphery were observed. The electron-transparent regions immediately inside the cell wall were observed only in the cells in the logarithmic growth phase; their nature and the mechanism of formation are now under investigation

of 0·5–1·2μ in diameter, occurring singly, in pairs or in clumps. It was non-motile, but flagella-like structures were observed by electron microscopy (Fig. 1). Growth occurred well on nutrient agar, blood agar and especially well on 'Tween'-agar[2] under aerobic conditions, developing smooth, flat and greyish-white colonies. Sugar fermentation was very poor: glucose, arabinose, xylose and galactose gave slight acid formation only after long incubation (2–4 days). Simmons's citrate test was positive; urease was negative. Nitrates were not reduced to nitrites; catalase was positive; indole was not formed; gelatin was not liquefied within 2–3 weeks.

In general, taxonomic characteristics of this organism resembles closely *Neisseria catarrhalis* (Frosch and Kolle) Holland[5], but it differs in its habitats and some of its sugar fermentations. So this organism belongs to the Micrococcaceae rather than to the Neisseriaceae, but no description corresponding to this organism has been found in Bergey's system[5]. I believe it is a new strain of Micrococcaceae, and propose the name *Micrococcus tweenis* for it. (The details of the taxonomic description will be presented elsewhere.)

Fig. 2 shows the growth-curves of this organism grown in synthetic media[1] containing 'Tween 80' as a sole source of carbon. The higher the concentration of 'Tween 80' used, the more abundant was the growth; and if no 'Tween 80' was added, no growth was observed.

'Tween 80' (0·05–0·8 per cent) stimulated the oxygen uptake of the cell suspension prepared from an 18-hr. culture grown on 'Tween'-agar medium. The oxygen uptake was also stimulated vigorously by such long-chain higher fatty acids and substances related to the tricarboxylic acid cycle such as oleate, stearate, palmitate, acetate, succinate, malate, fumalate, glutamate and aspartate, and to some extent by citrate and lactate.

Details of investigations with this organism will be reported elsewhere.

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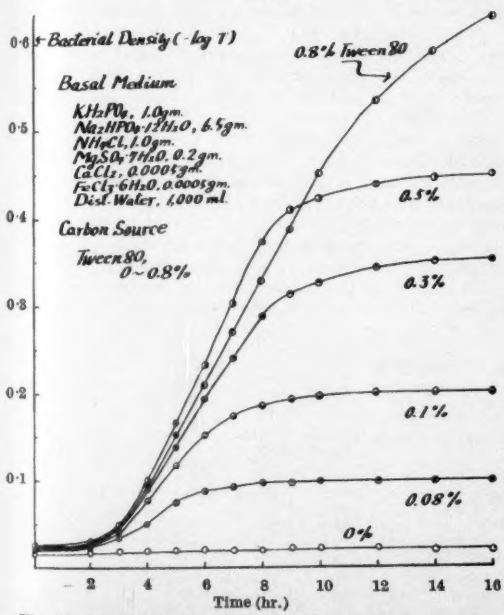

Fig. 2. Growth curves of a newly isolated coccus grown in synthetic media containing 'Tween 80' as sole source of carbon. The concentration of 'Tween 80' was varied from 0·08 to 0·8 per cent. The culture was shaken at about 55 strokes per min. at 37° C.

Polyploidy in Bluebells

IN my preliminary account[1] on polyploidy in bluebells (*Endymion nonscriptus* (L.) Garcke, and *E. hispanicus* (Mill) Chouard), I mentioned that some of the huge forms in gardens which I found to be triploids looked very much like the central one of Turrill's coloured figures[2]. Owing to the absence of any caption on Turrill's figures, I was led to take this huge central figure as the putative hybrid between the two species which Turrill mentions in his text. A few months after my publication, Turrill[3] explained his figures and pointed out that the central figure was *E. hispanicus*; he also suggested that since I did not examine his own material cytologically the material which I examined and found to be triploid might be different from his in ploidy.

I therefore asked for some of his material of *E. hispanicus*, most of which on cytological examination I found to be triploid. Thus it seems there is little doubt but that the huge bluebell represented by the central figure of Turrill's coloured figures is the triploid of *E. hispanicus* and not the ordinary (diploid) form.

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Micro-fungi in Pine Litter

Although a certain amount of work has been carried out on the microbiological characteristics of horizons in podzol soils, there is as yet little relating to the subdivisions of the A_0 horizon. In any such work there are obvious advantages in studying litter derived from a single species. Plantations of *Pinus sylvestris* have the added advantage that slow decomposition of the litter leads to the accumulation of a considerable A_0 horizon readily divisible into distinct layers. The A_0 horizon of forest soils can be subdivided in the following way: *L* layer, undecomposed litter; *F* layer, decomposing but still recognizable litter; *H* layer, amorphous humus. In cases of considerable accumulation the *F* layer may be further subdivided into *F*1, needles dark in colour, often still intact, and *F*2, needles greyish, fragmentary, compressed[1].

Samples of the *L*, *F*1 and *F*2 layers of the A_0 horizon of the podzol at Delamere Forest, Cheshire, were selected by inspection and transported to the laboratory in separate, sterile containers. Preliminary work with the standard dilution plate technique proved unsatisfactory, and records of the fungi on the needles were made therefore by two methods: (1) direct observation of freshly sampled needles; (2) observation of needles rigorously washed to remove surface-borne spores and maintained on a nutritive medium for two weeks. The type of result obtained is shown in Table 1. The abundance of the different fungi is expressed as the percentage of the total number of needles on which each fungus was observed.

Table 1 demonstrates both qualitative and quantitative differences in the fungal populations of the three subhorizons investigated. Direct observation showed maximal fungal activity to occur on needles of layer *F*1. Numerous fruiting structures of several fungi not represented on nutritive media were also seen in this layer. In layer *L* only *Lophodermium pinastri* was regularly observed, but in layer *F*1 three species other than *Lophodermium* occurred with very high frequency. *Verticicladium trifidum* has been recorded by Hughes[2], but the *Helicoma* species will be described elsewhere as a new species, and isolate 1/6 will be described elsewhere as a new genus. The two new forms are darkly pigmented, and produce reticula on the needle surface from which the conidiophores arise. Layer *F*2 showed mutilated remains of these fungi, a result of the activities of mites and other microfauna. Basidiomycete mycelium was, however, observed most frequently in this layer.

The cultural method of isolation revealed several fungi not seen by direct observation. Certain of these, that is, *Antennularia*, sterile dark isolate 3/10 and *Fusicoccum* were seen to occur with decreasing frequency from layer *L* to layer *F*2, while there was a corresponding increase in the occurrence of *Trichoderma* and *Penicillium* species. Little indication of this clear-cut succession was obtained by the use of the dilution plate technique. The above data are based on isolations carried out in March 1957, but comparable differences between the subhorizons have been recorded for all other months.

I wish to express my thanks to Prof. A. Burges and Dr. D. Parkinson for their guidance during this work, which was carried out during the tenure of an Agricultural Research Council Research Studentship.

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Protogynous Hermaphroditism in *Coris julis* L.

Coris julis L. and *C. giofredi* Risso have long been regarded as separate species, although Steindachner[1] claimed *julis* to be the male and *giofredi* the female individuals of the same species. In the Bay of Naples Lobianco[2] found mature male and female individuals both in *C. julis* and in *C. giofredi* and therefore rejected Steindachner's hypothesis of sexual dimorphism in *Coris*.

Work on this subject was resumed in the summer of 1956 following reports of transformation of *giofredi* into *julis* individuals in the basins of the Leghorn Aquarium (Razzauti). As hermaphroditism has been shown to be widespread among teleosts[3], researches were undertaken upon the hypothesis of the existence of false gonochorism[4] in *Coris*.

Statistical and histological investigations were conducted on 329 samples taken during August 1956–January 1957 from populations living at a depth of between 8 m. and 30 m. in the vicinity of Leghorn.

Typical, fully grown individuals in the *julis* livery have a mean length of $14{\cdot}81 \pm 0{\cdot}32$ cm. and they are distinguished by a marked prominence of the first three rays of the dorsal fin and by a prominent tooth at the proximal end of each upper jaw. Their colours are very bright: the dorsal region is blue-green, a long deep orange band runs from the opercular region to the tail and a short black strip extends under it from the attachment of the pectoral fin to a region below the eleventh dorsal ray. The sides of the ventral region are white or shaded with light blue.

Table 1

Method	Layer *L*		Layer *F*1		Layer *F*2	
	Spp. recorded	Frequency (per cent)	Spp. recorded	Frequency (per cent)	Spp. recorded	Frequency (per cent)
Direct observation	*Lophodermium pinastri*	35	*Helicoma* sp.	92	*Basidiomycete* mycelium	55
			Verticicladium trifidum	89		
			Isolate 1/6	89		
			Lophodermium pinastri	40		
			Basidiomycete mycelium	12		
Washing and plating on nutritive agar	*Antennularia* spp.	75	*Trichoderma viride*	75	*Trichoderma viride*	100
	Isolate 3/10	60	*Antennularia* spp.	40	*Penicillium* spp.	45
	Fusicoccum bacillare	35	*Penicillium* spp.	25	*Antennularia* spp.	5
	Trichoderma viride	35	Isolate 3/10	10		
			Fusicoccum bacillare	10		

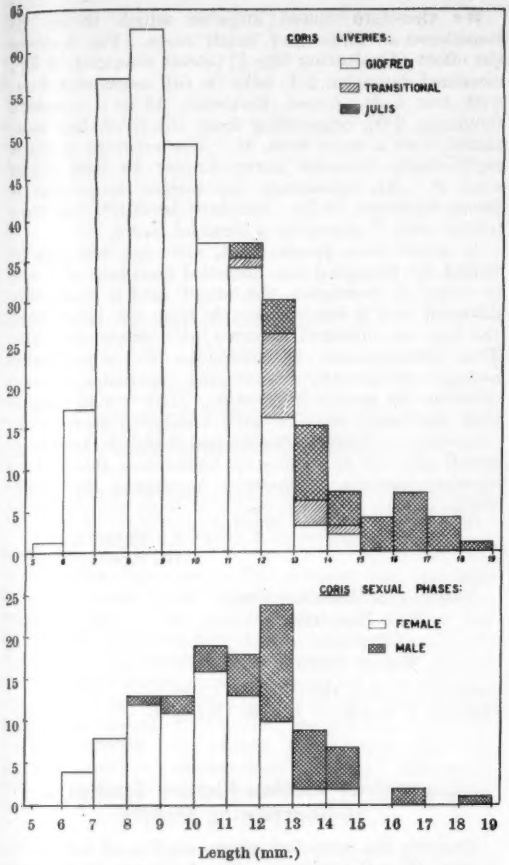

Fig. 1. Frequencies of *Coris* individuals in the different liveries (above) and in male and female phase (below) at various lengths (in millimetres)

Conspicuous black and red spots are evident between the first three rays of the dorsal fin.

Individuals in the *giofredi* livery have a mean length of $9{\cdot}29 \pm 0{\cdot}57$ cm. The above characters are missing, the dorsal region is brown and a golden band is visible on each side of the ventral region.

Intermediate characters are found in individuals with a mean length of $12{\cdot}76 \pm 0{\cdot}10$ cm.: the first *julis* character which appears among smaller transition individuals is a prominence of the first three dorsal rays, and the teeth at the ends of the upper jaws are the last character to be established in *julis* individuals (Fig. 1, above).

Gonads are reduced during the months from August to January and therefore individuals were sexed by histological methods. The examination of 118 paired gonads showed that smaller specimens (mean length $10{\cdot}14 \pm 0{\cdot}22$ cm.) are females, bigger specimens (mean length $12{\cdot}75 \pm 0{\cdot}30$ cm.) are males. A few big individuals in the *giofredi* livery (15·9 per cent) are nevertheless males. Transitional individuals are mostly (66·6 per cent) males (Fig. 1 below). Thus the livery appears to be partly independent of sex-conditions. Only a few specimens showed signs of a preceding female phase (small and degenerating oocytes) in gonads where most germinal cells were beginning spermatogenesis. These individuals (which were classified in the male phase) were either large sized *giofredi*, transition forms or small sized *julis*. Young individuals were all females and they showed the *giofredi* livery.

No regional preference of male and female germ cells in the reversing gonad has been detected so far, and the developing male germ cells rapidly replace oocytes after their expulsion or degeneration. For this reason gonads appear generally in pure female or male conditions.

Investigations are now being conducted with samples both from Leghorn and from the north-western coast of Sardinia in order to study the whole annual cycle and sex variation in *Coris*. Nevertheless the following conclusions can be drawn from the present results: (*a*) *C. giofredi* and *C. julis* are the younger and older individuals of a single protogynous hermaphrodite species (or intersexual species according to D'Ancona's terminology) that must be named as *Coris julis* L.; (*b*) since the sex change from female to male phase runs roughly parallel to the change of the livery, the female and the male phases in most individuals (but not in all of them) are represented by the *giofredi* and the *julis* livery respectively; (*c*) as a supposed 'sex dimorphism' is widespread among members of the Labroid family, it may indicate the existence of false gonochorism among other species of the group.

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Size Variation in *Entamoeba histolytica*

The human bowel is inhabited by amoebæ with the morphology of *Entamoeba histolytica*, but of differing mean size. The larger of these can be tissue-invading pathogens or may, usually in a smaller form, remain as a commensal in the bowel[1]. Further, there is considerable overlap in size range, at least in the commensal form, with a still smaller amoeba of similar morphology which has variously been known as *E. hartmanni* Prowazek, 1912; *E. histolytica hartmanni* Hoare, 1949; or as the small race of *E. histolytica* of Dobell, 1919. Of these views, the latter two have been the most widely favoured, but the work of Burrows[2] has indicated that there are morphological differences which suggest that the small amoeba should be considered a distinct species. However, there is, even on Burrows's criteria, still overlap with the large form, certain features being found sometimes in one and not in the other and vice versa.

Recently, we encountered a stool in which there were, fairly obviously, two different sizes of amoebæ, both with *histolytica* morphology. In a culture, this difference continued over a period of months. Fig. 1 shows a histogram based on the diameters of 600 live trophozoites from such cultures and shows a superimposed normal distribution from the hypothetical mean (4·6 micrometer units—10 micrometer units = 25·75μ) and its standard deviation (1·89). The

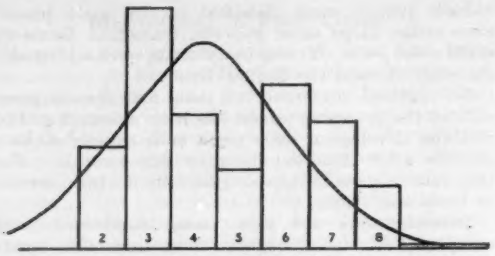

Fig. 1. Histogram of line *Z* based on the diameter of 600 live trophozoites with a superimposed normal curve based on the hypothetical mean and its standard deviation (10 micrometer units = 25·75μ)

bimodal curve actually found indicates that there were clearly two different types present.

In the course of experimental work to be reported elsewhere on the isolation of amoebæ from human liver abscesses, we noted that the size of these amoebæ is apparently governed by the type of bacterial associate provided. Fig. 2 shows three frequency polygons showing the size of lineal descendants from the same specimen (*L*) of liver pus. *L*.68 (mean diameter 20·8μ; standard deviation 3·0) was isolated in culture with *Clostridium welchii* as associate; *L.M.* (mean diameter 15·4μ; standard deviation 2·9) was isolated by the provision of a mixed flora *M*. The difference in size is significant, being more than three times its standard error ($P < 0{\cdot}001$). When, however, these lines were mixed and cultured together the resultant curve was unimodal (mean diameter 16·9μ; standard deviation 2·3) and lay between the two parent curves, showing that the difference in size was due to environmental effects.

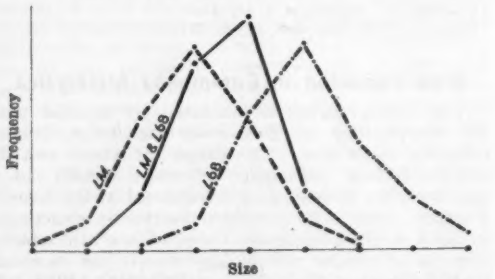

Fig. 2. Size-frequency polygons in lines *L*.68 and *L.M.* and in their combined culture *L.M.* and *L*.68, which gives a unimodal curve

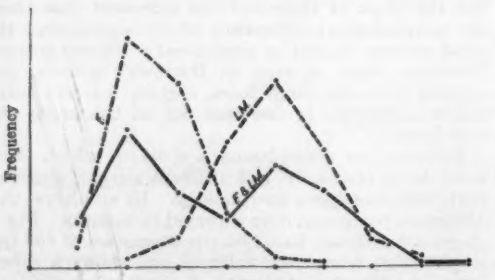

Fig. 3. Size-frequency polygons in lines *F* and *L.M.* and in their combined culture *F* and *L.M.*, which gives a bimodal curve indicating that *F* and *L.M.* segregate

We therefore mixed amoebæ which would be considered as 'large' and 'small' races. Fig. 3 shows the effect of culturing line *F* (mean diameter, 9·3μ; standard deviation 2·1) with its full associated flora, with line *L.M.* (mean diameter, 15·4μ; standard deviation 2·9), originating from the liver, but associated with a stool flora *M*. The mixture shows a significantly bimodal curve similar to that found with *Z*. An apparently commensal large line *B* (mean diameter, 19·2μ; standard deviation 3·1) when mixed with *F* also gave a bimodal curve.

It would thus appear that, although size can be varied by changing the bacterial associate of a line of 'large' *E. histolytica*, the 'small' race is genetically different and is distinguishable from the large when the two are cultured together in a single test-tube. This phenomenon is analogous to reproductive isolation in sexually reproducing organisms, a valid criterion for specific separation. This would suggest that the small amoeba with *histolytica* morphology occurring in human stools, and hitherto known as 'small race' or *E. histolytica hartmanni*, should be a separate species, *Entamoeba hartmanni* Prowazek, 1912.

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Lapse-time Motion Picture Studies of Soil-burrowing Insects

During the past few years, studies of the movements of soil-burrowing insects have been made using radioactively tagged insects[1]. Originally, the method involved finding the position of maximum response of a manually operated portable Geiger tube and ratemeter, but later a machine was developed which would record automatically the movement of a radioactively tagged insect such as a wireworm[2]. The sensing element in this machine was a thyrode tube (Victoreen 1*B* 85), rotated about a vertical axis at the end of a fixed arm. Translatory movements of the axis of Geiger revolution were recorded on an operating surface by a stylus.

The possibility of using this machine for obtaining motion-picture studies of insect movements has been investigated. In effect, the wireworm, by means of the tag and the machine, was provided with a pencil. The movements of the pencil and machine were photographed, using lapse-time technique to give a speeded-up picture of the worm movements.

A wireworm was tagged and liberated in a tray of soil placed on the floor. A table was placed over the tray and the machine set on the table. A sheet of glass was placed over the machine. A piece of grease pencil was attached to the machine on the Geiger axis and held in contact with the under-surface of the glass by a spring. The path taken by the wireworm was thus traced on the lower side of the glass sheet (Fig. 1).

A 16-mm. ciné camera was mounted above the machine so as to view it through the glass sheet. The camera was equipped for solenoid-controlled

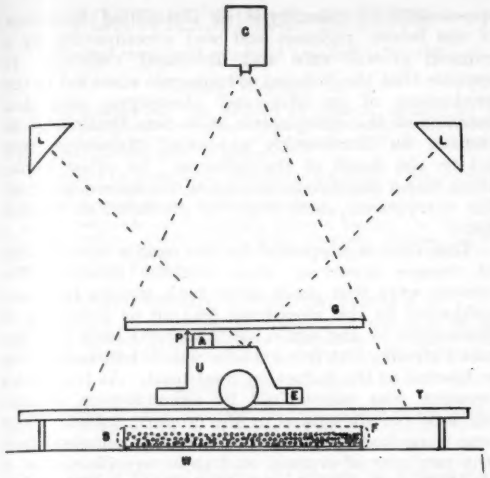

Fig. 1. Equipment used for lapse-time pictures. *C*, camera; *L*, lights; *G*, glass sheet; *P*, wri ing stylus; *U*, tracking unit; *T*, table; *S*, tray of soil; *F*, plastic moisture seal; *W*, tagged wireworm; *A*, clock; *E*, camera timing unit

single-frame exposures. The solenoid was operated by a clock-controlled microswitch mounted on the tracking unit. The exposure interval chosen was 30 sec. and pictures were taken over a 3-hr. period. The result was essentially a speeded up ciné film of the wireworm's movement in a period of 3 hr. (Fig. 2). The particular wireworm involved travelled at the rather high speed of 23 in./hr. As it turned out, the whole operation was rather abbreviated and the exposure interval chosen was too long. However, the short film obtained serves to demonstrate the value of the technique.

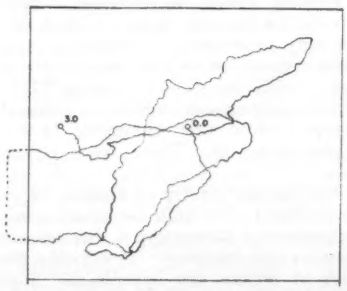

Fig. 2. Copy of the final trace obtained in lapse-time test. Figures give the time in hr. after the worm was introduced. The part of the trace indicated by a broken line was outside the field of view of the camera for a short time. Soil layer 15 in. × 22 in. × 1 in.

We are grateful to the Dominion Department of Agriculture, Division of Entomology, for generous financial assistance.

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An Assemblage of Arthropod Remains from a Roman Occupation Site at St. Albans

In the course of an excavation of a Roman occupation site at St. Albans, Hertfordshire, in 1955, a ditch was discovered which had been partly filled in, during the first century A.D., with a large quantity of organic material. Among this mass, bracken, grass, moss, leaves, twigs, seeds, etc., could still be recognized, and at one point it reached a depth of 7 ft. At least one wooden building had been erected on top of this; but when it had fallen into disuse the site had been levelled off by dumping clay and earth on it to a depth of about one foot. Afterwards a succession of substantial stone houses had been built, between the first and fourth centuries A.D., forming a succession of strata about 8 feet thick, which sealed this ditch-filling in an undisturbed state until the present time.

Among the organic material in this deposit there was found a colony of living earthworms (*Eophila oculata* Hoff.)[1] which may have persisted there since Roman times, and another interesting find was the large number of insect and other arthropod remains in quite a good state of preservation. Of about 60 specimens recovered, 40 could be identified at least to the family, and many to the genus, and, tentatively, to the species. The following is a shortened list of the specimens, identified as nearly as possible:

Insecta
- Dermaptera: *Labia minor* L. (many specimens)
- Lepidoptera: *Arctia caja* L. (puparium)
- Coleoptera
  - Carabidae:
    - *Clivina fossor* L.
    - *Nebria brevicollis* F. (several specimens)
    - *Feronia madidus* F.
  - Hydrophilidae:
    - *Megasternum obscurum* Marsh.
  - Histeridae:
    - *Hister* spp.
    - *Onthophilus striatus* Forst
    - *Abraeus granulum* Er.
  - Staphylinidae:
    - *Staphylinus* sp.
    - *Homalota* sp.
    - *Philonthus* sp.
  - Scarabaeidae:
    - *Aphodius* sp.
  - Cucujidae:
    - *Monotoma spinicollis* Aube.
  - Tenebrionidae:
    - *Tenebrio* (cf. *obscurus* F.)
- *Hymenoptera:* Fragments of an ant

Acarina
- *Fuscuropoda marginata* Koch.
- *Tectocepheus velatus* Mich. and George.
- *Oppia nitens* Koch.
- *Scheloribates laevigatus* Koch.

Fine structures such as setæ, wing scales of Lepidoptera, and tracheæ were well preserved, and although pigmentary colours had either blackened or faded, structural colours were as bright as ever. The ditch contents presented a very moist and strongly reducing environment, as shown by the presence of quantities of vivianite (a hydrated ferrous phosphate), and such conditions may be important for the preservation of chitinous arthropod parts.

The layer of clay and earth which had been dumped on top of the deposit had trapped an ecological assemblage of insects, etc., from which conclusions about the nature of the deposit may be drawn. First of all, it represents what one would expect to encounter in a refuse heap, with incidental species entering by chance from neighbouring grassland habitats. Insect remains were found at all levels in the deposit, although they were most plentiful in the upper part. This suggests that the deposit was accumulated

during a fairly short period of time, a few years at the most, as the arthropod cuticle does not withstand prolonged weathering.

The species found in the lower levels suggest the presence of rotting vegetation; but at the top large numbers of puparia of *Lucilia* and *Calliphora* (the bluebottle and greenbottle flies) indicated the presence of carrion and dung. It is interesting to note that the layer of clay which sealed the whole deposit was probably dumped on a day in late summer. The assemblage of species trapped immediately under it would suggest this, and also, although most of the fly puparia were vacated, a few clusters still contained remains of the immature flies.

For the identification of the specimens I am indebted to Mr. J. Balfour-Browne, and Dr. Owen Evans of the British Museum (Natural History), and Prof. F. E. Zeuner of the Institute of Archæology, University of London. I must also thank Mr. John Lunn of the Verulamium Museum for making the material available to me.

P. L. Bradley

69 Manor Road,
Barnet, Herts.

[1] Dobson, R. M., and Satchell, J. E., *Nature*, 177, 796 (1956).

A Case of Limitation of Phenotypic Variation

Jinks[1] suggests that where phenotypic variation is caused by a cytoplasmic system, the range of phenotypic expression attainable by the system would have quite definite limits imposed by the system itself (cytoplasmic limits) or, by the non-viability of phenotypes lying outside these limits (physiological limits). Cytoplasmic limits would result where further changes in cytoplasmic content are impossible, as when all cytoplasmic elements involved in the change are lost. Physiological limits would result where although more extreme changes in the cytoplasm are possible they would lead only to non-viable phenotypes. Jinks's results suggested a physiological basis to limits of variation obtained in *Aspergillus glaucus*.

Results from *Neurospora crassa* provide an example of a physiological limit to what appears to be cytoplasmically determined variation in the expression of a certain genotype. This genotype, which differs from the wild type by at least two genes, can show two different extremes of expression. At one extreme the culture forms no protoperithecia and instead produces many discrete areas of dark brown pigment (*bk* phenotype), while at the other extreme there is no sign of the dark pigment and weakly developed protoperithecia, which may be functional, are produced in limited parts of the culture (*S* phenotype). Stages intermediate between these two are formed. Cultures initiated from ascospores can show any stage in the range of phenotypes; but subcultures derived from them almost invariably show intermediate or *S* phenotypes only. The *bk* phenotype can be induced to appear, or reappear in subcultures by certain treatment, when its appearance, and later disappearance after the treatment are typical of the behaviour of a dauermodification. This and other evidence suggest that the phenotypic range results from the interaction of the genotype with varying cytoplasmic states or conditions. Prolonged continuation of the treatment used for inducing the appearance of phenotype *bk* intensified formation of the brown pigment and was accompanied by a reduced growth rate and decreased viability. It appears that the induced cytoplasmic state led to the production of an abnormal phenotype, and that increase of the cytoplasmic state was limited by its causing an increasingly abnormal phenotype that led to the death of the cultures. In other words, there was a physiological limit to the extent to which the cytoplasmic state could be produced or selected for.

This view is supported by the results of a number of crosses involving these mutant strains. The crosses were first made after both strains had been subjected to the treatment leading to induction of phenotype *bk* and again seven months later with the same strains, but from stocks which had never been subjected to the induction treatment. At both times crossing was carried out by simultaneous inoculations of parent strains in the crossing tubes. There was a marked difference in the results obtained from the two sets of crosses, and these are illustrated in Table 1 with data from a typical cross.

Table 1. Ascus Types produced by Cross 3-1 *A* × 5-7 *a*

	Ascus types						No. of asci
First crossing	*L L*	*L L*	*bk*	*bk*	—	*bk*	2
	L L	*L L*	—	*bk*	—	*bk*	2
	L L	*L L*	*bk*	*bk*	—	—	3
	L L	*L L*	*bk*	—	—	—	2
							9
Second crossing	*L L*	*L L*	*S*	*S*	*S*	*S*	6
	L L	*L L*	*S*	*S*	*S*	*bk*	3
	L L	*L L*	*S*	*S*	*bk*	*bk*	1
							10

L, Wild type protoperithecia; *bk*, *bk* phenotype; *S*, Intermediate and *S* phenotypes; —, non-viable ascospore.

The cross was such that a 1 : 1 segregation of wild type to mutant would be expected. The mutant segregants in the progeny from the first crossing all had a strongly expressed *bk* phenotype, and 50 per cent of the mutant ascospores were non-viable. In the progeny from the second crossing 87·5 per cent of the mutant segregants showed an intermediate or *S* phenotype, while the remaining 12·5 per cent showed phenotype *bk*. There were no non-viable ascospores.

In all the crosses strong expression of phenotype *bk* was associated with high ascospore non-viability, while expression of phenotype *S* was associated with low ascospore non-viability. It appears that in the first series of crosses the cytoplasmic state of the progeny was such as to produce the extreme *bk* phenotype, and that in many of the ascospores this cytoplasmic state was intensified to beyond the limit of phenotypic viability. In the progeny of the second series of crosses a more normal cytoplasmic state existed and led to a considerable amount of protoperithecial formation in the mutant segregates, and complete ascospore viability.

This work was carried out largely at the Department of Genetics, University of Adelaide, while I held a New Zealand National Research Fellowship.

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[1] Jinks, J. L., *Proc. Roy. Soc.*, B, **146**, 527 (1957).

FORTHCOMING EVENTS

*(Meetings marked with an asterisk * are open to the public)*

Monday, February 10

BRITISH SOCIETY FOR THE HISTORY OF SCIENCE, PHILOSOPHY OF SCIENCE GROUP (in the Joint Staff Common Room, University College, Gower Street, London, W.C.1), at 5.30 p.m.—Prof. M. Ginsberg: "Evolution, Development, Progress".

INSTITUTION OF ELECTRICAL ENGINEERS (at Savoy Place, London, W.C.2), at 5.30 p.m.—Discussion on "The Herbert Report and the 1957 Act", opened by Mr. E. R. Wilkinson.

ROYAL INSTITUTE OF CHEMISTRY, LONDON SECTION (at Norwood Technical College, Knight's Hill, West Norwood, London, S.E.27), at 7 p.m.—Dr. J. Green: "Recent Advances in the Chemistry and Function of the Vitamins".

Monday, February 10—Saturday, February 15

DEPARTMENT OF SCIENTIFIC AND INDUSTRIAL RESEARCH (at the Institution of Civil Engineers, Great George Street, London, S.W.1), at 10 a.m. daily—Exhibition—"Road Research in the Colonies".

Tuesday, February 11

INSTITUTION OF CIVIL ENGINEERS (at Great George Street, London, S.W.1), at 5.30 p.m.—Mr. A. R. Collins and Mr. D. R. Sharp: "Design and Construction of Concrete Roads Overseas".

INSTITUTION OF THE RUBBER INDUSTRY, LONDON SECTION (at the Royal Society of Tropical Medicine and Hygiene, 26 Portland Place, London, W.1), at 5.30 p.m.—Mr. J. Mann: "Moulding". 7 p.m.—Mr. B. Ellis and Mr. G. N. Welding: "Characteristics of Accelerated Sulphur Vulcanization of Natural Rubber".

ROYAL INSTITUTE OF CHEMISTRY, LONDON SECTION (joint meeting with the SIR JOHN CASS COLLEGE CHEMICAL SOCIETY, at the Sir John Cass College, Jewry Street, London, E.C.3), at 6.30 p.m.—Prof. C. K. Ingold, F.R.S.: "An Organic-Chemical Contribution to the Inorganic Chemistry of Nitrous Acid".

Wednesday, February 12

ROYAL SOCIETY OF ARTS (at John Adam Street, Adelphi, London, W.C.2), at 2.30 p.m.—Sir Gavin de Beer, F.R.S.: "The Inhabitants of Switzerland".

UNIVERSITY OF LONDON (at Guy's Hospital Medical School, St. Thomas's Street, London, S.E.1), at 5 p.m.—Prof. C. D. Waddington, F.R.S.: "Evolutionary Adaptation to Environmental Stress".*

GEOLOGICAL SOCIETY OF LONDON (at Burlington House, Piccadilly, London, W.1), at 5 p.m.—General Meeting.

BEDFORD COLLEGE (at Regent's Park, London, N.W.1), at 5.15 p.m.—Dr. D. M. MacKay: "The Brain as an Information System".*

INSTITUTE OF PHYSICS, EDUCATION GROUP (at 47 Belgrave Square, London, S.W.1), at 5.30 p.m.—Discussion on "The Graduateship Examination". Speakers: Prof. J. G. Wilson, Mr. A. E. De Barr Prof. M. R. Gavin, Mr. J. Yarwood and Mr. N. Clarke.

CEMENT AND CONCRETE ASSOCIATION (at the Institution of Structural Engineers, 11 Upper Belgrave Street, London, S.W.1), at 6 p.m.—Dr. S. Champion: "Grouted Concrete Construction".

INSTITUTION OF ELECTRICAL ENGINEERS (at the Central Hall, Westminster, London, S.W.1), at 6 p.m.—Mr. R. Ledger: "The Electrification of Railways" (Faraday Lecture prepared by the late Mr. G. H. Fletcher).

SOCIETY OF CHEMICAL INDUSTRY, FOOD GROUP—NUTRITION PANEL, (at 14 Belgrave Square, London, S.W.1), at 6.15 p.m.—Third Meeting on "Some Nutritional and Allied Problems Confronting the Food Manufacturer". Dr. C. G. Sumner and Dr. F. Aylward: "Emulsifying and Dispersing Agents".

BRITISH ASSOCIATION OF CHEMISTS, LONDON SECTION (at the Wellcome Building, 183 Euston Road, London, N.1), at 7 p.m.—Assistant Divisional Officer Dennett (London Fire Brigade): "Fire Prevention".

RADAR ASSOCIATION (in the Anatomy Theatre, University College, Gower Street, London, W.C.1), at 7.30 p.m.—Sir Robert Watson Watt, F.R.S.: "The Early Days of Radar".

SOCIETY FOR VISITING SCIENTISTS (at 5 Old Burlington Street, London, W.1), at 7.30 p.m.—Discussion Meeting on "Science in General Education". Speakers: Dr. A. W. Barton, Mr. Richard Fort, M.P., and Sir Frank Ewart Smith, F.R.S.

BRITISH PSYCHOLOGICAL SOCIETY, SOCIAL PSYCHOLOGY SECTION (at Birkbeck College, Malet Street, London, W.C.1), at 8 p.m.—Papers on "Recent Developments in Advertising". Dr. Donald Melvon: "Motivation Research"; Dr. N. F. Dixon: "Sub-threshold Advertising".

Thursday, February 13

ROYAL SOCIETY (at Burlington House, Piccadilly, London, W.1), at 4.30 p.m.—Mr. B. C. Abbott, Dr. A. V. Hill, F.R.S., and Mr. J. V. Howarth: "The Positive and Negative Heat Production Associated with a Nerve Impulse".

UNIVERSITY COLLEGE, LONDON (in the Physiology Theatre, Gower Street, London, W.C.1), at 5 p.m.—Dr. H. F. C. Crick: "The Nucleic Acids".* (Further Lectures on February 20 and 27.)

UNIVERSITY OF LONDON (in the Hatton Lecture Theatre, Queen Mary College, Mile End Road, London, E.1), at 5.30 p.m.—Prof. M. W. Humphrey Davies: "Electrical Engineering in the Future" (Inaugural Lecture).*

ROYAL AERONAUTICAL SOCIETY (at the Institution of Mechanical Engineers, Birdcage Walk, Westminster, London, S.W.1), at 6 p.m.—Mr. H. L. Cox: "The Application of the Theory of Stability in Structural Design".

SOCIETY OF CHEMICAL INDUSTRY, MICROBIOLOGY GROUP (in the Department of Botany, Imperial College, London, S.W.7), at 6.30 p.m.—Meeting on "The Enzymic Degradation of Carbohydrates".

CHEMICAL SOCIETY (in the Large Chemistry Lecture Theatre, Imperial College of Science and Technology, South Kensington, London, S.W.7), at 7.30 p.m.—Prof. B. Lythgoe: "Some Recent Advances in the Chemistry of the Vitamins D" (Tilden Lecture).

Friday, February 14

INSTITUTE OF PHYSICS, NON-DESTRUCTIVE TESTING GROUP (at 47 Belgrave Square, London, S.W.1), at 6 p.m.—Mr. E. L. Ripley: "The Analysis of Wreckage in Aircraft Accident Investigation".

ANTIQUARIAN HOROLOGICAL SOCIETY (at the Science Museum, South Kensington, London, S.W.7), at 7 p.m.—Mr. W. J. Gazeley: "Clock Escapements".

SOCIETY OF CHEMICAL INDUSTRY, FINE CHEMICALS GROUP (in the Chemistry Lecture Theatre, King's College, Strand, London, W.C.2), at 7 p.m.—Mr. F. W. Thomas: "Fine Chemicals in the Textile Industry".

BRITISH GLACIOLOGICAL SOCIETY (in the Geology Department, The University, Oxford), at 8.30 p.m.—Mr. W. H. Ward: "Glaciology and Mountaineering".

ROYAL INSTITUTION (at 21 Albemarle Street, London, W.1), at 9 p.m.—Prof. J. Z. Young, F.R.S.: "The Structure of Cells".

Saturday, February 15

BIOCHEMICAL SOCIETY (in the Beveridge Hall of the Senate House, University of London, W.C.1), at 10.30 a.m.—Symposium on "Glutathione".

BRITISH PSYCHOLOGICAL SOCIETY (in the Gustave Tuck Theatre, University College, Gower Street, London, W.C.1), at 2.30 p.m.—Dr. Desmond Morris: "An Analysis of Paintings and Drawings by Chimpanzees".

APPOINTMENTS VACANT

APPLICATIONS are invited for the following appointments on or before the dates mentioned:

ASSISTANT SECRETARY (graduate of a British university, preferably in science or engineering, with some experience of industry) to the UNIVERSITY APPOINTMENTS BOARD—The Registrar, The University, Liverpool (February 15).

PHYSICIST, Senior grade (with an honours degree in physics and previous experience in hospital physics, preferably in the application of radioisotopes to clinical problems), IN THE ISOTOPE LABORATORIES—The Personnel Officer, St. Thomas's Hospital, London, S.E.1 (February 15).

LECTURER (Grade I or II) IN APPLIED MATHEMATICS—The Registrar, The University, Bristol 8 (February 21).

LECTURER or ASSISTANT LECTURER IN THE DEPARTMENT OF AERONAUTICAL ENGINEERING—The Secretary and Registrar, The University, Southampton (February 22).

LECTURER or ASSISTANT LECTURER IN THE DEPARTMENT OF ZOOLOGY—The Secretary and Registrar, The University, Southampton (February 22).

LECTURER (preferably with experience with electrical machines and an interest in physical phenomena in machines) IN THE DEPARTMENT OF ELECTRICAL ENGINEERING—Prof. A. Tustin, Electrical Engineering Department, Imperial College, London, S.W.7 (February 22).

SCIENTIFIC OFFICER (with an honours degree in zoology) IN THE NEMATOLOGY DEPARTMENT—The Secretary, Rothamsted Experimental Station, Harpenden, Herts (February 22).

SENIOR LECTURERS or LECTURERS (2) IN PHYSICS at the University of Melbourne, Australia—The Secretary, Association of Universities of the British Commonwealth, 36 Gordon Square, London, W.C.1 (Australia, February 28).

CHAIRS OF PHILOSOPHY, BIOCHEMISTRY, PHARMACOLOGY, AGRICULTURE and MECHANICAL ENGINEERING in the University of Ceylon—The Secretary, Association of Universities of the British Commonwealth, 36 Gordon Square, London, W.C.1 (Ceylon, March 1).

LECTURERS or ASSISTANT LECTURERS (2) IN THE DEPARTMENT OF PHYSICS—The Secretary and Registrar, The University, Southampton (March 1).

TUTORIAL RESEARCH STUDENT IN MATHEMATICS (PURE OR APPLIED)—The Principal, Royal Holloway College, Englefield Green, Surrey (March 1).

FARM DEMONSTRATOR (with a degree in agriculture or an honours degree in pure science followed by an adequate postgraduate qualification in agriculture, and preferably with some training in field experimentation), for duties which will include farm classes on the University Farm to undergraduate and postgraduate students—The Secretary, School of Agriculture, The University, Cambridge (March 5).

LECTURER or ASSISTANT LECTURER IN THE DEPARTMENT OF PHILOSOPHY—The Secretary and Registrar, The University, Southampton (March 8).

LECTURER (with appropriate qualifications in chemistry, and preferably a working knowledge of geology and mineralogy) IN GEOLOGY (GEOCHEMISTRY)—The Secretary, The University, Edinburgh (March 8).

LECTURER IN GEOLOGY at the University College of Ghana—The Secretary, Inter-University Council for Higher Education Overseas, 29 Woburn Square, London, W.C.1 (March 10).

LECTURER IN PHYSIOLOGY—The Secretary, The Queen's University, Belfast (March 15).

RESEARCH ASSISTANTS (2) (with research experience) IN THE DEPARTMENT OF THEORETICAL PHYSICS, for special research in fundamental particle and nuclear physics—The Registrar, The University, Liverpool (March 15).

LECTURER IN TECHNICAL MATHEMATICS—The Secretary, The University, Edinburgh (March 28).

LECTURER IN ARCHÆOLOGY; ASSISTANT LECTURER IN SOCIAL ANTHROPOLOGY and a DEMONSTRATOR IN PHYSICAL ANTHROPOLOGY—The Secretary of the Appointments Committee, Faculty of Archæology and Anthropology, The University, Downing Street, Cambridge (March 31).

LECTURER or ASSISTANT LECTURER IN THE DEPARTMENT OF ELECTRONICS—The Secretary and Registrar, The University, Southampton (March 31).

IMPERIAL CHEMICAL INDUSTRIES RESEARCH FELLOWS IN CHEMISTRY, PHYSICS, ENGINEERING, PHARMACY, CHEMOTHERAPY and related subjects—The Secretary, Academic Council, Queen's University, Belfast (April 1).

PROFESSOR AND HEAD OF THE DEPARTMENT OF DAIRYING—The Dean, Faculty of Agriculture, University of Alberta, Edmonton, Alberta, Canada (April 1).

IMPERIAL CHEMICAL INDUSTRIES FELLOWS FOR RESEARCH IN PHYSICS, CHEMISTRY, COLLOID SCIENCE, BIOCHEMISTRY, ENGINEERING, METALLURGY, PHARMACOLOGY, CHEMOTHERAPY or related subjects—The Secretary-General of the Faculties, The Old Schools, The University, Cambridge (April 30).

LECTURER IN FUEL TECHNOLOGY AND CHEMICAL ENGINEERING—The Registrar, The University, Sheffield (May 1).

LECTURER IN FOREST BOTANY—The Secretary, The University, Edinburgh (May 15).

AGRICULTURAL ECONOMIST (with a university degree in agricultural economics, agriculture, or economics, and practical experience in the organization of the export of horticultural produce, local marketing, and distribution problems) to act as Marketing Adviser to the Maltese Government—The Director of Recruitment, Colonial Office, London, S.W.1, quoting BCD.63/19/04.

ASSISTANT LECTURER IN PHYSICS—The Registrar, The University, Nottingham.

BIOCHEMIST, Basic grade (with a science degree preferably with honours in biochemistry or chemistry)—The Secretary, Horton General Hospital, Banbury, Oxon.

EXPERIMENTAL OFFICER (with a degree in chemistry or agricultural chemistry or equivalent qualifications), to carry out chemical analyses of soils and plants—The Secretary, Rothamsted Experimental Station, Harpenden, Herts.

FISHERIES OFFICER (with a university degree in biology with emphasis on zoology, and experience of fisheries work) in the Northern Region of Nigeria, to execute plans for the development of river fisheries, and the demonstration and organization of peasant fish culture—The Director of Recruitment, Colonial Office, London, S.W.1, quoting BCD.65/408/01.

NATIONAL RESEARCH COUNCIL OF CANADA POST-DOCTORAL FELLOWS IN THE DEPARTMENT OF CHEMISTRY—Dr. R. P. Graham, Chairman, Department of Chemistry, McMaster University, Hamilton, Canada.

RESEARCH ASSISTANT (preferably graduate in chemistry, biochemistry or physiology) IN THE DEPARTMENT OF PATHOLOGY, for work in connexion with steroid investigation—The Clerk to the Governors, St. Bartholomew's Hospital, London, E.C.1, marking envelope "Research".

RESEARCH OFFICER (SOILS) with a university degree in geology or natural science and at least two years postgraduate experience in tropical soils research) IN THE FOREST RESEARCH DEPARTMENT, Federation of Nigeria, to initiate and carry out investigations and studies of forest soils—The Director of Recruitment, Colonial Office, London, S.W.1, quoting BCD.61/14/07.

REPORTS and other PUBLICATIONS

(not included in the monthly Books Supplement)

Great Britain and Ireland

Scottish Home Department. Marine Research, No. 3 (1957): The Food and Feeding Habits of Larval and Post-Larval Herring on the Northern North Sea. By Dr. R. N. Bhattacharyya. Pp. 15. (Edinburgh and London: H.M. Stationery Office, 1957.) 7*s*. net. [1811

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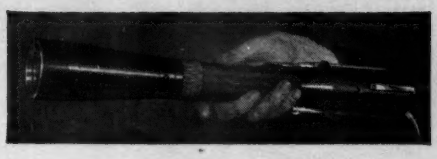

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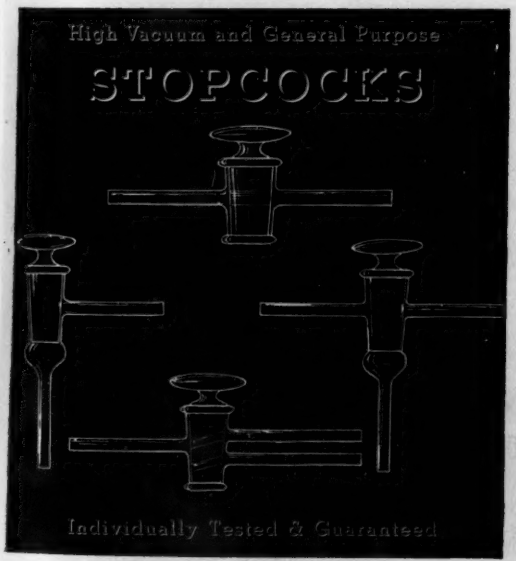

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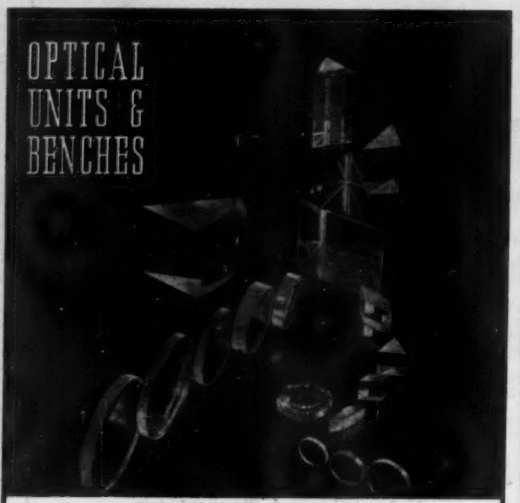

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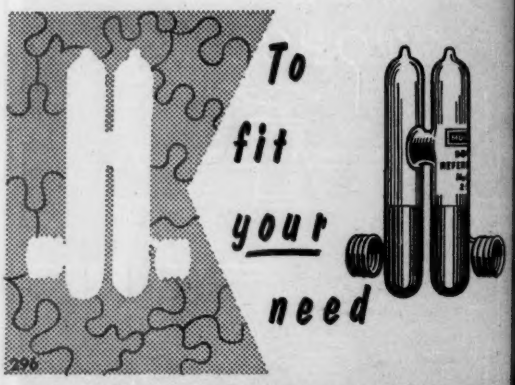

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